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The Development of Social Stratification in Bronze Age Europe[1]

by Antonio Gilman

THE STRATIFICATION OF EUROPEAN BRONZE AGE SOCIETIES has been taken for granted since the beginning of research into their material remains over a century ago. The burials which make up the bulk of the evidence leave no doubt that marked social inequalities emerged during the 3d and 2d millennia B.C. Although some earlier studies have attempted to reconstruct Bronze Age social structure in Europe (e.g., Otto 1955), it is only recently that much detailed attention has been paid to either descriptive or theoretical aspects of how social stratification came into being (Gilman 1976; Kempisty 1978; Randsborg 1973, 1974; Renfrew 1972; Shennan 1975; Wüstemann 1977). These studies strongly suggest that the elites of the European Bronze Age were hereditary. The Early Bronze Age cemetery at Branč in Slovakia, for example, had numbers of rich subadult graves (Shennan 1975), the lack of possible achievements of the deceased suggesting that their superordinate status was ascribed (cf. Binford 1971). The increase in the proportion of rich female to rich male burials over the course of the Early Bronze Age in Denmark (Randsborg 1974) may be interpreted as reflecting the progressive separation of high status from achievement, since the importance of female activities relative to male ones is unlikely to have increased over that time.[2] Specific studies such as these confirm what has long been accepted on the basis of more general considerations. Thus, the development of metallurgy, a specialized technology mainly for the manufacture of display items, involves an elaborate system of production and exchange and thereby suggests the existence of a permanent upper class to consume the goods so arduously brought into being. The broad geographic distribution of elite artifact styles such as bell beakers and (in a later period) swords likewise points to the existence of upper classes whose recruitment was sufficiently stable for them to establish a web of widespread, mutually supportive partnerships. Indeed, the very passage from collective to "individualizing" burial rituals, a change occurring at the start of the Bronze Age over much of Europe, suggests the development of social stratification (Renfrew 1976). In their recent survey of Bronze Age Europe, Coles and Harding (1979:535) conclude:

> During the course of the Bronze Age a number of important changes took place—changes that lend the period its characteristic appearance and distinguish it from anything that had gone before. . . . Perhaps the most obvious of these is the rise of the privileged. . . . It is hard to think of this process in terms other than those of aggrandizement of the few, the rise of the elite, and the start of social stratification.

The scarcity of studies of later prehistoric social organization in Europe is, no doubt, in part attributable to pessimism concerning the possibility of dealing with questions of social structure using archaeological data (Hawkes 1954). It is also in part due to the wide acceptance of a coherent theory of how and why social stratification arose in later prehistoric Europe, a theory which obviated any need to pay close attention to the internal dynamics of social history in Europe itself. The clearest statement of this outlook is in the later works of Childe (1956, 1958). Childe's view was that Oriental power and knowledge had transformed Europe in later prehistoric times much as European power and knowledge had transformed the world under capitalism. Oriental centers would have sought raw materials, in particular metals, from Europe and would have provided the initial capital to stimulate a network of commodity exchange based on metallurgy. Referring to the Copper Age of southeastern Spain, for example, Childe (1957:284) in a typical passage argued that "the urbanization of the Almerian economy . . . is presumably a reflection, however indirect, of Oriental cities' demand for metal." The fortunes of local elites in Europe would have depended essentially on Near Eastern events. This widely shared theory, as much as anything else, was responsible for restricting research on the European Bronze Age to typological studies capable of demonstrating links to the Orient. Understanding the development of social stratification required no detailed consideration of the workings of prehistoric political economy.

Increasing uneasiness with diffusionist arguments (e.g., Clark

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[2] Randsborg suggests that the increasing wealth of female burials relative to male ones may be due to an increasing importance of women's work in farming. However, as Neustupný (1967) points out, the plow agriculture of the Bronze Age would tend to increase the importance of male, not female, work in agriculture.

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1966) and demonstrations of the independence of European cultural features supposed to be of Near Eastern derivation, such as megaliths (Renfrew 1967) and metalworking (Renfrew 1969), have combined to bring about the collapse of the traditional theory of culture change. This collapse has largely been an empirical one, caused by radiocarbon determinations, spectrographic analyses, and other matters of fact. As a result, there is something of a theoretical vacuum in European prehistoric studies. How is the emergence of elites to be explained without Near Eastern intervention? The main candidate for a new "paradigm" is the functionalism put forward by Renfrew (1972, 1973*a*) and other prehistorians of the younger generation. My purpose in this paper is to show that functionalist formulations will not explain the development of social stratification in Europe and to suggest an alternative theory to account for the rise of dominant social strata in prehistoric European societies of the Copper and Bronze Ages. Mutatis mutandis, this nonfunctionalist account will be seen to have broad applicability to similar instances of social change beyond Europe.

FUNCTIONALIST APPROACHES TO THE DEVELOPMENT OF SOCIAL STRATIFICATION

The rise of complex, hierarchical societies presents itself as an evolutionary advance. Complex societies have larger populations than their egalitarian predecessors and deploy more powerful productive forces. Once established, they tend to expand at the expense of less populous and hierarchical neighbors, thus illustrating the "principle of competitive exclusion" (Carneiro 1978). It is hardly surprising to see the state described as exhibiting "greater maturity in an ecological succession" of political types (Gall and Saxe 1977:260). The adaptive effectiveness of hierarchy in moderating environmental and social uncertainty is so widely accepted in the recent anthropological literature that scholars who question it feel it necessary to decry "evolutionism" as a whole (Yoffee 1979). What is at issue, however, is the usefulness of functionalism for understanding how class societies come into being.

A shared feature of the few archaic states for which adequate documentary sources exist is a hereditary nobility: *alii* (Hawaii), *pilli* (Aztec), *orejones* (Inca), etc. Membership in these groups is by ascription and grants a small minority wealth disproportionate to their numbers (i.e., preferential access to resources). These unquestionable ruling classes pose a clear problem for conventional accounts of the emergence of complex social organization. Harris (1971:393), for example, clearly expresses his uneasiness at putting forward a functionalist account of the origins of social stratification: "What were the rewards of those who were cut off from the two-million-year-old heritage of free access to resources? . . . Why was control of soil, water, and even the air yielded up into the hands of a relatively small group of people?" To these rather difficult questions most anthropologists (including Harris) give a disarmingly simple answer: ruling classes obtain their position because they provide services essential to the mass of their subjects.

Most of the differences between theories about the origins of complex societies revolve around the sorts of services which the elites would have provided in particular situations. Mesopotamia, Egypt, China, and other centers lend prima facie support to Wittfogel's hydraulic hypothesis: "The handling of relatively large amounts of water . . . requires coordination of a communal labor force and, above a certain magnitude, a leadership that directs the construction and maintenance of hydraulic installations and the distribution of irrigation water" (Wittfogel 1972:70). Earle (1978:37–49) elucidates several functional variants of this managerial approach. The diversity of resources in regions such as Mesoamerica supports the redistribution theory put forward by Sahlins (1958:5): "As dispensers of food and other goods, and in reward of their logistic support of the community, chiefs gained in prestige and extended their political and ceremonial prerogatives." Flannery and Coe (1968) and Rathje (1971), for example, represent internal and external variants, respectively, of this approach. As Earle (1978:5) points out, although Carneiro (1970) presents his resource-circumscription/warfare theory as a "conflict" model it in fact has a strong functionalist component: the population can only secure access to needed resources through superior (i.e., more hierarchical) military organization (cf. Webster 1977). Service (1978:32) sums up the consensus of recent scholarship: "Redistribution (and especially trade), military organization and public works were all basic in the classical civilizations, but all must have had small beginnings in the simple attempts by primitive leaders to perpetuate their social dominance by organizing such benefits for their followers." It is the possibility (indeed, the likelihood) of the co-occurrence of more than one "prime mover" that leads Flannery (1972) to recast these approaches into the more general language of information theory. In this version elites constitute "higher-order regulators" of the information needed for the functioning of a complex society. Social stratification comes into being, then, because in one or more ways "the chief creates a collective good beyond the conception and capacity of the society's domestic groups taken separately. He institutes a public economy greater than the sum of its household parts" (Sahlins 1972:140).

Most recent work on the development of the European Bronze Age does not confront the question of causes directly, but, as Neustupný (1976:246) points out, a new consensus has begun to emerge concerning the processes of social change which that development involves. The most explicit and extensive instance of the new view is Renfrew's use of the redistribution variant of the functionalist argument outlined above to explain the emergence of social stratification in Greece and the Aegean around 2000 B.C. (Renfrew 1972:chap. 18). The Minoan and Mycenaean palaces would have been the focal points of activities contributing to the general welfare: their princes encouraged trade, crafts were improved (leading to "new metal tools increasing agricultural efficiency"), and foodstuffs were more effectively made available to primary producers, who were stimulated to increase their output "by the wish to receive redistributed goods" (p. 490). Renfrew (1973*a*:210) sums up: "The redistribution of goods, which is organized and controlled by the chief himself, . . . is, of course, exactly the function fulfilled by the palaces of Minoan-Mycenaean civilization, taking in and storing the produce from the very different fields, orchards, and pastures which are found, even in a small area, in south Greece." Similar "individualizing chiefdoms" are suggested for Bronze Age Wessex and elsewhere in Europe. A similar view has been extended to the interpretation of trade networks of the preceding period: in the Late Neolithic, stone axes and coppers are the kula-like *vaygua* whose exchange provided channels to "carry a much greater volume of subsistence products" (Sherratt 1976:568). Clarke's (1976) discussion of beakers as primitive valuables is in the same vein. The part played by elites in the process of social change in later prehistoric Europe is often left somewhat unspecified in these studies, but the range of citations, if nothing else, gives a clear indication of the increasing acceptance by Europeanist prehistorians of a functionalist account of the emergence of superordinate social strata (cf. Milisauskas 1978). The new paradigm which is proposed to replace the *ex oriente lux* account of the European Bronze Age elite may be summarized as follows: the development of extensive networks for the procurement and allocation of resources necessary for everyone led to the emergence of a permanent ruling class, which managed the complex production/distribution problems involved.

The functionalism of the authorities cited above involves three steps, the first two of which are anthropological commonplaces. First, a culture is regarded as an integrated whole ("a system with subsystems"). Second, this system is seen as what permits those involved in it to survive ("culture as extrasomatic means of adaptation"). Third, particular features of the system are explained as being adaptive. This last step is a dangerous one which tends towards a Panglossian acceptance of the actual as the necessary (cf. Friedman 1974). In particular, when this third step is taken to explain what the elite does in a stratified society, severe misunderstandings arise.

The functionalist account of the development of elites may be criticized at once for its failure to explain the hereditary character of the class of "higher-order regulators." Even if one grants that certain economic situations demand leadership for the common good, it does not follow that the rulers must be recruited from a ruling class. It is not apparent that the best way of choosing efficient managers is by birth. A classic defect of functional explanations is their inability to account for possible alternatives (Hempel 1959).

Quite apart from such logical knots, the functionalist account does not match what we actually know about the part played by the ruling classes of stratified societies. The central propositions of the functionalist account are that elites are in fact involved in managerial transactions, that these transactions confer an adaptive benefit upon the population as a whole, and that elites obtain their positions because they provide these benefits. These specifications are not met in the concrete cases to which the functionalist explanations are applied.

Wittfogel's hydraulic theory has stimulated several detailed studies of the relation between irrigation and social complexity (see the critical review of the literature by Earle 1978). On the whole, these studies cast doubt on the theory's crucial managerial component. Where irrigation is extensive or important, elites are often not involved in the distribution of water. Thus, Glick (1970) shows that the extensive irrigation systems of mediaeval Valencia were built and operated by the cultivators themselves. Adams (1965) indicates that early Mesopotamian irrigation systems, while crucial to agricultural production, were on a scale entirely compatible with local control and management. In Dynastic Egypt, according to Butzer (1976), the upper tiers of this indubitably stratified society had no managerial functions with respect to the distribution of the Nile's water: the flood-basin systems were operated at the local level, with only ceremonial intervention by the pharaoh. Even where elites (through their representatives) do administer local hydraulic systems, their intervention may not be generally beneficial. In Hawaii irrigation was indeed supervised by appointees of the chiefs, but this direction was not required by the technical complexity of these small, simple networks. Earle (1978:141) sums up the situation as follows: "Who were the most direct beneficiaries of managerial activities? For whom did the managers work? . . . Their main role was specifically to mobilize and to direct labor activities so as to maximize the income flow of the elites." In most instances elites are not involved in the management of irrigation systems. Where they are, it is mainly in their own interest rather than on behalf of the social whole.

The redistribution and warfare variants of the functionalist account of social stratification are stronger than the hydraulic variant in that the empirical evidence for elite involvement in directing these activities in person or through representatives is unquestionable. What may be doubted is whether these activities are adaptive—whether they contribute to the general welfare. Cowgill (1975:506) puts matters succinctly: "People in strong positions have often promoted, and even believed, the argument that 'What's good for me is good for the system, and what's good for the system is good for everybody,' but the difficulties with this idea hardly need spelling out." Earle's examination of redistribution in its classic instance, Hawaii, is instructive. Although the islands' ecological diversity is supposed to make organized exchange between regions (arranged through the chief's good offices) an adaptive arrangement, local communities "were laid out so as to minimize differences in the resources available to their populations" (Earle 1977:223). The exchanges between regions which did take place were by direct barter and not through channels controlled by the chiefs. Redistribution is supposed to benefit primary contributors because they can become part of a larger economic network run by an elite. In Mesopotamia, for example, one is told that the Sumerian elite administered a "Great Organization" needed, among other things, to import basic raw materials, such as wood and stone, which were scarce on the Tigris/Euphrates floodplain (Lamberg-Karlovsky and Sabloff 1979:179). The clay sickles that are so characteristic a feature of Mesopotamian artifact assemblages as early as Ubaid times are mute testimony to how little was actually distributed to the primary producers. A parallel argument can be mounted against the warfare variant of the functionalist theory. Warfare, directed by elites, is beneficial in functional terms because it supplies scarce resources, such as land, to the victors. If concrete historical cases are any guide, however, very few of the spoils accrue to the mass of the population whose contributions support the military enterprise. The conquests of the Roman Republic provide a well-documented example (Anderson 1974:67–68; cf. Finley 1973:55–56):

> The senatorial aristocracy profited enormously from the financial sacking of the Mediterranean that succeeded progressive annexations by Rome, making boundless fortunes in tribute, extortion, land and slaves; but it was utterly unwilling to provide even a modicum of compensation to the soldiery whose fighting yielded these unheard-of gains. . . . To have paid them bounties would have meant taxing the possessing classes, and this the ruling aristocracy refused to consider.

The redistribution and warfare variants of the functionalist account are stronger than the hydraulic variant only because taxes and booty are more direct than agricultural improvements as avenues to elite self-aggrandizement.

It is undeniable, of course, that ruling classes may sometimes be of service to their subjects by directing public works, encouraging commerce, helping in the event of disasters, and so forth. Such activities may be useful means by which the elite can consolidate, extend, and legitimate its wealth and power, but they are not responsible for its attainment of power. Marx (1967 [1887]:322) puts the point clearly enough: "It is not because he is a leader of industry that a man is a capitalist; on the contrary, he is a leader of industry because he is a capitalist."

If these theories about the origins of elite are doubtful in general, they seem even more unsuited to an explanation of the origins of stratification in Europe. In the cases for which these theories were developed, managerial functions are at least plausible: there are cities, large public works, etc. In later prehistoric Europe, virtually the only evidence for social complexity is the wealth of the elites themselves. Bronze Age settlements, for example, are extremely scarce over much of Europe, a circumstance which does not suggest they were large. In regions where more settlements are known, such as southeastern Spain, Copper and Bronze Age sites usually cover a hectare or less. An exception is Los Millares, whose settlement may cover four hectares; in Europe this passes for "urbanism" (Arribas 1959). In fact, it is notable that settlement hierarchies, which are often taken to be the prime archaeological indicators of higher-order regulation (Wright and Johnson 1975, Isbell and Schreiber 1978), are definitely attested in Europe only in those regions involved in demonstrable commerce with Mediterranean

civilizations: the Aegean in the Bronze Age and Central Europe in the Early Iron Age (Frankenstein and Rowlands 1978, Wells 1977).[3] That Mediterranean trade was so important to Minoan/Mycenaean and Hallstatt/La Tène florescence gave strength, of course, to the *ex oriente lux* theory of European prehistoric development. This empirically falsified account cannot be replaced by functionalist approaches, if only because in prehistoric Europe it is not apparent what positive functions there were to be regulated. Precisely for this reason, prehistoric Europe provides an excellent vantage point from which to envision a nonfunctionalist theory of the origins of social stratification.

A NONFUNCTIONALIST ALTERNATIVE

The question posed by functionalists in their explanation of the origins of stratification is "What services do elites provide for society?" The opposite attack on the problem begins by asking: "In spite of the fact that their actions do not serve common interests, how do elites establish and maintain their control?" This problematic is implicit in a number of works on the origins of social complexity. In his discussion of the "Urban Revolution" Childe (1951) emphasized the need to concentrate a surplus for the support of nonproducers and saw clearly that the nonproducers capture that surplus in their own interest. A similar recognition of the realities of social stratification is clear in the work of Adams when he points out that irrigation's contribution to the development of elites is its "encouragement of differential yields" (Adams 1966:72; cf. Diakonoff 1969) or that early trade is more fruitfully viewed as serving "the interests of the agents of exchange" than as fulfilling "broad social needs" (Adams 1974:242; cf. Kohl 1978). Earle's (1978) reconsideration of the organization of chiefdoms in Hawaii shows clearly how the greed of the elite quite adequately explains their dealings with their subjects. The desire of the *alii* to enhance their political power made them extract the maximum possible surplus by encouraging their subject populations to produce more and by conquering yet more people. The elites sometimes found it advisable to assist commoners (for example, by helping them rebuild irrigation systems after natural disasters), but it is clear that this was only to ensure a future source of income. Systemic benefits could have been secured at lesser cost to the mass of the population. In short, the rise of an elite can be understood without reference to the common good. What needs to be explained is how elites acquire and maintain their power in spite of the fact that, much of the time, their actions are against the interests of the mass of the population.

The conditions permitting elites to establish themselves permanently become clear when one looks at the internal dynamics of social systems without a ruling class. The literature on tribal (rank) societies makes it plain that there is no want of aspirants to superordinate status. In Siuai, to choose a characteristic example, the would-be leader achieves and maintains his ambitions by demonstrating his abilities as a warrior, ceremonial leader, food producer, etc.; "numerous cases were recorded wherein ambitious fathers or maternal uncles have wasted their resources and effort to push forward young men" lacking the requisite combination of attributes (Oliver 1967:441). The inability to pass on leadership within a hereditary line is a consequence of the ease with which supporters can shift their allegiance from a leader who proves unsatisfactory. An egalitarian social order is maintained as such by the facility with which a leader, actual or potential, can be abandoned by his followers, should he displease them. Carneiro (1968:136) has pointed out how this same weakness in "internal political controls" leads to village schisms. In a sense, then, one may consider lineage segmentation and concomitant village fission to be the ultimate mechanism by which the self-aggrandizement of "big-men" is checked. We seek to understand how and why the attempts of ambitious tribesmen to secure hereditary, ascribed leadership posts succeeded in Copper and Bronze Age Europe. We must look, therefore, for conditions which would retard the process of segmentation characteristic of tribal societies.

A common explanation for decrease in segmentation is population increase and consequent pressure on spatially limited resources ("resource circumscription"). The strength of this argument is that it explains how dissidents can afford the courage of their convictions in tribal societies: it is ecologically possible for them to escape ("land suitable as a habitat for a dissident group is easily found" [Carneiro 1968:136]). Whatever the empirical merits of this argument may be in other settings, it will not explain the retardation of segmentation in prehistoric Europe. Broad stretches of uninhabited, but habitable, wilderness existed in Europe and the Mediterranean well into the mediaeval and early modern period. In later prehistoric times, when population densities must have been far lower, there would have been plenty of land into which people could move to avoid unwanted masters. The shift towards social complexity occurs, furthermore, on too broad and diverse a front for the resource-circumscription argument to be viable.

Any nonfunctionalist account of the development of social stratification must confront the central functionalist idea that hierarchy ultimately is adaptive for society as a whole. In general terms, the functionalist position is that elites retard segmentation (attract a following) by providing managerial services required in a highly productive economy: in the Mesopotamian case, for example, the elite would organize, in the area of exchange, the procurement of wood and stone and, in the area of production, the construction and maintenance of irrigation works. The nonfunctionalist must turn this around and explain in nonmanagerial terms why societies with highly productive economies tend to have elites. In other words, what aspects of the production and exchange systems of Copper and Bronze Age Europe opened up the opportunity for effective long-term exploitation by a ruling minority?

COMMODITY EXCHANGE

The basic nonfunctionalist argument on the role of exchange in the origins of class societies goes back to Engels (1972 [1891]). For Engels craft production, and especially metallurgy, entailed the development of a network of commodity exchange, control of which gave middlemen the opportunity to establish positions of wealth and power. This idea was, as we have seen, taken up by Childe in his account of European Bronze Age social change. Childe tied his theory to hypothetical Near Eastern prospectors and merchants, but one could easily allow local factors more play.

In order to use trade as a motive process for the emergence of social stratification, one must argue that the goods exchanged are essential ones. The goods which the middleman can deny the household which refuses to pay his price must be required for the household's livelihood. In other words, trade must involve, directly or indirectly, the basic subsistence sector of the economy. Thus, Adams (1966) shows that specialization in agricultural production (and the consequent need to exchange foodstuffs) promoted the development of social inequalities in Mesopotamia. Kohl (1978) suggests that highlanders in the

[3] Milisauskas (1978:156, 229) argues that, as of the 3d millennium B.C., a two-tiered settlement hierarchy is attested in areas where there is adequate evidence for settlement patterns (e.g., Funnel-Beaker Poland). The differences in settlement size may, however, be attributable to differences in the time spans of site occupation, the richness of local resource bases, or other nonhierarchical factors. Even in so thoroughly surveyed an area as Late Bronze Age northwestern Bohemia (Bouzek, Koutecký, and Neustupný 1966), it is difficult to discern a clear ranking of settlements.

Near East became dependent on grain imports from the lowlands and thus on their suppliers. Childe (1951, 1954) stressed the importance of metal tools (made from exotic raw materials by a few specialists) in facilitating land clearance and harvesting. A nonfunctionalist account of the importance of exchange in the origins of European Bronze Age elites could, in principle, be constructed along these lines without Oriental intervention.

The difficulties attendant upon using these arguments in a European setting are not theoretical, but empirical. Foodstuffs are bulky: to move a sufficient volume of them to create local or regional dependence on their importation would have been quite beyond the capacity of Bronze Age transport systems. Furthermore, the available economic evidence does not support the hypothesis of extensive trade in subsistence items. If in the Bronze Age "smoked fish from the Baltic would have made a useful contribution to inland diets" (Coles and Harding 1979: 281), one should find appropriate fish bones on inland sites. One does not, and their absence need not be attributed to taphonomic or sampling biases. All the animal and plant remains from Bronze Age sites are consistent with the reasonable view that their inhabitants ate foods produced or foraged locally. It is hard to envision Aunjetitz or Argaric commoners dependent on rations from afar and submissive to the chiefs who controlled their supply.

Quite apart from the simplifications involved in some formulations about the role of smiths and prospectors as agents of trade (Rowlands 1971), the metallurgical variant of the commodity-exchange argument faces the difficulty that, on present evidence, it is hard to see how the metal implements actually known from Bronze Age Europe would have helped increase overall production substantially. What Arribas (1968:49) says of Iberia—"we know of no agricultural tools of metal in the Bronze Age"—is not quite true for Europe as a whole, but it is not far off the mark. Only in the Late Bronze Age are substantial numbers of utilitarian metal artifacts found. The very fact that most bronzes are found in burials and votive hoards suggests that metal had a social and ideological rather than a practical value. A luxury like metal reflects, of course, the differential possession of wealth, for which the material may serve as a convenient form of storage. By providing equipment both prestigious and suitable for trade for other luxuries (cf. Harrison and Gilman 1977), metallurgy may have consolidated the sway of an already existent elite. It would not seem, however, that copper and bronze played a significant role in maintaining the economic and social security of households. Accordingly, it is hard to accept that it called the elite into being. It is better to see metal as an index than as a cause of the development of social stratification in Europe.

While the commodity-exchange theory of elite origins may be useful in other settings, in later prehistoric Europe it founders on the apparent self-sufficiency of local communities. Trade was mostly confined to luxuries.[4] We must look, therefore, to the processes of subsistence production themselves rather than to exchange for the material roots of elite origins.

[4] Coles and Harding (1979:61–63) emphasize the importance of salt in the economy of the Bronze Age. Since it is both biologically necessary to its consumers and portable in quantities sufficient to satisfy demand, salt arguably would be a better centerpiece for the commodity-exchange theory of elite origins than either metal or food. Its exploitation in later prehistoric Europe is widespread (Nenquin 1961); in the Halle/Saale region artifacts used in salt boiling date to the Early Bronze Age (Matthias 1976). This association of early salt production with the rich (i.e., clearly stratified [Otto 1955]) Saxo-Thuringian Aunjetitz is suggestive but requires confirmation in other regions. Elsewhere, known briquetage sites are associated with remains of later periods, when social stratification was already long established. In any event, the invisibility of salt in the archaeological record makes it hard to assess its role in the economies of areas that imported it.

CAPITAL-INTENSIVE SUBSISTENCE TECHNOLOGY

Nonfunctionalists have tended to neglect the role of subsistence production in providing possibilities for long-term exploitation by a ruling minority. Once again it is Childe who has suggested a fruitful approach. Referring to the early development of irrigation systems, he writes: "All through the Near East the best sites were reclaimed with toil. Capital in the form of human labor was being sunk into the land. Its expenditure bound men to the soil; they would not lightly forego the interest brought in by their reproductive works" (Childe 1951:89–90). This idea may be extended from irrigation to any technology which substantially increases productivity through preparatory labor. Segmentation is only easy if those who leave can readily produce in the manner and at the levels to which they are accustomed. Departure must not involve the abandonment of substantial assets. If, for example, subsistence depends on slash-and-burn farming, one can effectively abandon an undesirable leader by bringing forward the time of a shift in cultivation soon to be undertaken in any event. Conversely, if the productive system requires a heavy preliminary investment of work, the producers will be reluctant to relinquish the restricted resources they themselves have created. Where irrigation or any other capital-intensive form of subsistence is crucial to production, one can only abandon an undesirable leader if one sacrifices the work expended to create facilities which increase or insure yields. Under conditions thus impeding segmentation, the ambitions of aspirants to high status will be harder to check. In this way, then, elites can form as more productive subsistence technologies develop without the elites' being required to organize the productive improvements. This theory is, of course, particularly useful for explaining social change in Copper and Bronze Age Europe, where elites seem to have arisen without managerial functions. What remains to be specified is what systems of production were developed in later prehistoric Europe of sufficient intensity to have retarded the fission of primitive social groups.

The generally accepted view of the agricultural history of Europe during the 4th through 2d millennia B.C. sees slash-and-burn farming as the initial agricultural form, followed by various intensifications over the course of time. There is overall a progression towards more powerful systems of production which is not only logical, but also supported by the available evidence (Green 1979). Information is too scattered to permit systematic regional reconstructions of the varied evolution of subsistence techniques in later prehistoric Europe. A number of widespread developments do involve, however, the substantial, durable labor inputs which, following the theory just outlined, would help to unbalance an egalitarian political economy. Plow agriculture, Mediterranean polyculture, irrigation, and offshore fishing will be discussed here in an attempt to specify the relationship between agricultural and social change.

PLOW AGRICULTURE

Use of the ard is widely attested by the end of the 3d millennium B.C. (Late Neolithic and Copper Age contexts). The evidence falls broadly into five categories. First, there are discoveries of the ards themselves. Examples from Hvorslev in Denmark and the Polada-culture site of Ledro in northern Italy date to the earlier 2d millennium B.C. (Battaglia 1943, Lerche 1968). Second, there are artistic representations, such as the depictions of ards in the rock art of southern Sweden (Glob 1951) and the southern Alps (Anati 1961), attributed to the Bronze Age, or the copper model of yoked oxen from Poznań (Poland) of Copper Age date (Jazdzewski 1965: pl. 9). Third, there are plowing marks noted underneath barrows. At the

South Street long barrow in southern England these criss-cross furrows are C^{14} dated to the early 3d millennium B.C. (Fowler and Evans 1967). While some of these examples have been reinterpreted as being the result of turf-cutting rather than plowing (Barker and Webley 1978:170), the widespread occurrence of these subsurface markings as of Late Neolithic times throughout northern Europe (Neustupný 1969) suggests that the plow was in use on appropriate soils well before 2000 B.C. Fourth, there are the widely noted "Celtic" field systems in the British Isles and northwestern Europe, fields whose lynchet boundaries seem to have been produced by plowing. Many examples in Great Britain can now be placed in the 2d millennium B.C. (Barrett, Hill, and Stevenson 1976, Drewett 1978, Fowler 1971, Thomas 1978), and in Ireland some of these enclosures are dated to the 3d millennium (Caulfield 1978). The high phosphorus levels in the soils of Bronze Age fields on Dartmoor suggest a manuring regime (Denford 1975). Fifth, there is faunal evidence for the animals which pulled the ards. Clear metrical evidence for castrated cattle is reported from the Swiss Bronze Age (Higham 1968), and claims for similar finds are made for contexts as early as the 3d millennium B.C. (Bökönyi 1974:116). The horse, an animal typically used for its traction rather than simply as a nutritional resource, is first domesticated in the 3d millennium B.C. and is abundant in the 2d (Bökönyi 1974:243–48). The sum of these diverse lines of evidence indicates that throughout Europe plow agriculture was firmly established by about 2000 B.C. or earlier.

The plow presents clear advantages to the farmer in comparison to the hoe. Animal traction increases the area a man can work (or enables him to cultivate the same area with less effort). At the same time, the plow turns the soil more effectively (for example, by reincorporating plant materials), thereby increasing yields and shortening the fallow cycle. In a Mediterranean climate the pulverization of the soil by the ard helps retain needed moisture. On appropriate soils even the light plows used in prehistoric Europe would permit a large increase in productivity.

This increase is obtained at a high initial fixed cost. Fields must be cleared more thoroughly than for swidden farming. The removal of stumps, once completed with no little effort, is a permanent asset. The farmer must also have animals to pull the plow. This traction power must be created by human effort in advance of production. "With plow agriculture . . . no direct relation is exhibited between labor currently invested in the land and output. . . . To say plow agriculture is to say labor stored in the ground, in animals and in equipment" (Gudeman 1977:580). The lynchets separating plots in prehistoric European field systems may be considered the fossil remains of property boundaries newly arisen under a system of intensive agriculture (Lancaster 1979:330) and reflecting fundamental changes in land tenure and inheritance patterns (Goody 1976).

Mediterranean Polyculture

Renfrew (1972) has emphasized the importance of Mediterranean polyculture in generating the agricultural surplus necessary for the support of Bronze Age Aegean civilization. Olive pits, charcoal from pruned olive branches, oil presses, and lamps all clearly indicate the cultivation of the olive by the 3d millennium B.C.; a comparable range of palaeobotanical and artifactual finds shows that the vine was domesticated at the same time (J. M. Renfrew 1973:125–34; Zohary and Spiegel-Roy 1975). The diffusion of vine and olive cultivation into the central and western Mediterranean is generally supposed to have occurred as part of the Greek colonization of the 1st millennium B.C. There is reason to believe, however, that these eminently useful cultivars may have been exploited in the West 1,000 years or more earlier (Gilman 1976:315–16). In Spain, for example, olive pits have been recovered from El Gárcel and Nerja (Late Neolithic), Ereta del Pedregal (Copper Age), and El Argar and Serra Grossa (Bronze Age); grape seeds are reported from Monte de la Barsella (Copper Age) and Serra Grossa (Aparicio Pérez 1976:199; Arribas 1968:44; Hopf 1971; Hopf and Pellicer Catalán 1970). Like their Aegean analogues, the chalices of the Argaric Bronze Age may have been meant for wine drinking. Precisely because wild vines and olives are indigenous to the entire Mediterranean basin, it is likely that their prehistoric cultivation was not restricted to the eastern sectors of that region. Fig and carob are other Mediterranean tree crops for which evidence of prehistoric cultivation exists (Aparicio Pérez 1976:197–200; J. M. Renfrew 1973:134–36).

Cultivating these tree crops helps the farmer in several ways. Olives and vines are complementary to the staple cereals and legumes. The fruit trees may be intercropped with annual harvests, and the schedule of work which they demand does not conflict with that of the other cultigens. Thus, olives and vines generate an absolute increase in productivity in the regions where they can be grown. As olive oil or wine, pickled olives or raisins, the crops lend themselves to long-term storage. Thus Mediterranean polyculture promotes the material security of the subsistence farmer.

For the purposes of my argument, however, the most important feature of Mediterranean polyculture is not the increase in productivity which it permits, but the transformation of property relations which it implies. As farming was introduced to prehistoric Europe, several new cultigens—oats and rye, for example—were developed. They improved crop yields in temperate climates (which is why they came to be cultivated) but did not change the dynamics of domestic production: as annuals, oats and rye have much the same labor requirements and storage potential as the wheat and barley they supplement. Tree crops, in contrast, present radically new technical requirements. Vine cuttings do not yield fruit until three years after they have been planted but produce for generations thereafter. Olives do not yield fruit for ten to fifteen years after planting, come into full production some twenty years later, and continue to give fruit for centuries. In the meantime, the trees must be pruned, the ground around them plowed. In other words, the farmer must invest a lot of work before he (or his heir) receives a return. Mediterranean polyculture constitutes a capital-intensification of subsistence.

Irrigation

Chapman (1978) has stressed the potential importance of irrigation for agriculture in the more arid sectors of Mediterranean Europe (cf. Gilman 1976:313–15). Direct evidence for prehistoric irrigation (remains of dams and ditches) is scarce. Balcer (1974) describes a Late Helladic dam near Tiryns in the Argolid; Schüle (1967) reports a Copper Age irrigation ditch at Cerro de la Virgen in southeastern Spain. Given the likelihood that recent irrigation systems will have obliterated ancient ones, more extensive verification of the importance of irrigation in southern Europe must rely on indirect evidence, such as the location of sites with respect to water resources. Thus, in Late Bronze Age Messenia sites are often located near springs and irrigation systems now in operation (Van Wersch 1972). Copper and Bronze Age sites in the arid sectors of southeastern Spain are located at the confluence of seasonal streams to maximize the potential for flood-water farming (Chapman 1978). The evidence remains to be developed, but it seems likely that simple forms of irrigation were widespread in Mediterranean Europe during later prehistoric times.

In regions of Mediterranean climate, irrigation is generally advantageous. The diversion of water onto fields supplements and stabilizes the irregular rainfall and makes it possible to grow crops in the summer dry season. In regions of extreme aridity, such as the Almería/Murcia region of southeastern Spain (the "Níjar Desert" [Meigs 1966:89–91]), irrigation is essential for regular agricultural production. By increasing and

insuring yields, irrigation promotes the material security of the subsistence farmer in Mediterranean Europe.

In southeastern Spain, where the possibility of prehistoric irrigation has been looked into most closely, it is apparent that hydraulic systems would have been small in scale. The floodwater farming systems currently in operation in the region (Vilá Valentí 1961) are essentially of Neolithic character, and nothing more complex need be suggested for their prehistoric predecessors. Irrigation cannot have demanded techno-bureaucratic management. Its significance for social stratification must, rather, be along the lines suggested by Childe: once a system has been gradually expanded, its dams, ditches, and terraces represent a considerable investment.

Offshore Fishing

Accounts of prehistoric fishing in Europe after the Mesolithic are scarce, Clark's (1977) discussion of the offshore fishing activities of megalith-builders in Atlantic Europe and Evans and Renfrew's (1966) description of tunny fishing at Saliagos in the Cyclades being salient examples. There is reason to believe, however, that fishing may have been more important to later prehistoric European economies than the scanty literature would indicate. Thus, in southern Scandinavia, the hundreds of ships depicted on Bronze Age rock carvings and artifacts (Brøndsted 1958:135–40, 176), as well as the cod and haddock remains recovered from one of the region's few excavated Bronze Age settlement sites (Thrane 1971:160), suggest prehistoric exploitation of a rich available resource. The evidence remains to be recovered and developed, but the richness of Atlantic, Baltic, and Mediterranean fisheries and the long tradition of exploiting marine resources suggest that offshore fishing may have made a substantial contribution to the material security of the residents of coastal Europe.

To the extent that fishing is carried out beyond the immediate shore, it involves progressively more elaborate technological assistance. A hook and line, a casting net, a leister are all fairly simple, but the larger boats and nets required for effective exploitation of offshore fisheries involve a very considerable investment of labor in advance of production. The 15-m sewn-plank boats of Bronze Age date from North Ferriby (Yorkshire) are estimated to have had a working lifetime of 50 years (Wright and Churchill 1965). Once again, if such technologies are important to a group's subsistence, that group is dependent upon capital investments to which continued access must be insured by social means.

DISCUSSION

I have set forth four capital-intensifications of subsistence introduced more or less widely in Europe in later prehistoric times. Other possibilities—Barfield's (1971:71) mention of agricultural terracing in Bronze Age northern Italy, for example—remain to be explored. The changes mentioned here share important features. All are simple technologies to institute; the tasks which they entail can be carried out within the scope of cooperation between households which may be presumed to exist normally within the domestic mode of production (Sahlins 1972). In addition, all contribute to the production security of households. The plow, olive trees, irrigation systems not only increase, but also (and more significantly) stabilize production. Thus, the adoption of these techniques may be understood without appeal to factors such as population pressure or resource depletion. Finally, the benefits conferred by the new methods are all achieved by preparatory labor inputs which, once expended, assist production over the long term. Developed farming and fishing entail the investment of much work in long-lasting assets which cannot easily be relinquished. The building of dams, clearing of fields, and planting of trees create a man-made landscape to which continued access must be insured if the production security for which the labor was expended is to be maintained.

Under appropriate historical and ecological conditions, the desire for material security led later Neolithic farmers to create productive works of long-term and general utility. These assets would be of value to others than their creators. Thus, capital-intensification of subsistence transfers the problem of security from the material to the social field. The investments of labor to insure future production would have to be defended. But the value of these same assets would dampen the potential for social fission, so that it would be difficult to check the aspirations of those to whom the defense had been entrusted. In the face of a protector whose exactions seem excessive, the household's choices are limited: it may abandon the asset for which it sought protection; it may find another protector (who may prove no less self-aggrandizing than his predecessor); or it may submit to the excessive exactions. Over the long term, these options consistently favor the protectors. In the end there would have arisen a permanent ruling class. Its main symbols of power and prestige—arms and flashy luxuries entirely appropriate to the elite's function in society—constitute the most salient feature of the Early Bronze Age from Aunjetitz to El Argar.

Verification of this account ultimately must depend on the reconstruction of detailed sequences of economic and social change in the various regions of Europe. Two implications can, however, be at least partially examined. Temporally, the theory suggests that in any given region the introduction of intensified subsistence techniques preceded the development of social stratification. Spatially, it suggests that at any given time the degree of stratification was more intense in areas in which capital-intensification of subsistence was either particularly necessary or particularly advantageous.

Available evidence meets the first of these two corollaries. In Denmark, for example, plow agriculture is widely attested in Corded Ware/Battle Axe contexts of the later 3d millennium B.C. (Seeberg and Kristensen 1964), yet stratification does not develop until the Early Bronze Age (Randsborg 1974). A similar sequence of economic and social events occurs in central Europe (Neustupný 1969, Otto 1955). Olive and vine cultivation is well established in the Aegean by Early Bronze Age 2, well before the Minoan/Mycenaean "takeoff" into social complexity (Renfrew 1972). Given the evidence for substantial climatic stability over the past 7,000 years in southeastern Spain (Chapman 1978), irrigation must have been practised by the Níjar Desert's earliest farmers in the 4th millennium B.C.; social stratification is first apparent in the El Argar culture of the 2d millennium (the burial patterns of the preceding Los Millares phase being characterized by "ranking" [Chapman 1977]). Offshore fishing has been suggested as an important subsistence activity by Clark (1977) for the megalith-builders of later Neolithic Atlantic Europe, well before Bronze Age developments. Capital-intensification of subsistence clearly precedes the emergence of elites in later prehistoric Europe.

The scarcity of detailed studies of Bronze Age social structure makes it more difficult to assess the spatial implications of my theory. For Early Bronze Age Denmark, Randsborg (1974) has shown that positive correlations exist between the number of graves in a region, the degree of stratification reflected by wealth differentials in cemeteries, and the traditional grain yields for that region. This is consistent with the suggestion that plow agriculture generates not only higher population densities, but also greater social inequalities. An exception to this trend occurs in northwestern Jutland, where population density and wealth concentration are much greater than would be predicted by grain yields. It is notable, however, that northwestern Jutland is adjacent to the rich Limfjord fishing grounds (Rasmus-

sen 1974), where offshore fishing would be highly productive and comparatively safe. Randsborg's detailed regional assessments of social structure and economic potential are as yet rare in European prehistoric studies. For the most part one must rely on more general distributional arguments in seeking confirmation of the theory. Thus the significance of Mediterranean polyculture in Aegean culture process is reflected, as Renfrew (1972:283) indicates, by the tendency of major Minoan/Mycenaean sites to be located in areas of large-scale present viticulture. A similar argument can be put forward for arid southeastern Spain, whose Copper/Bronze Age sequence (the Los Millares and El Argar cultures) is the richest in the Iberian peninsula. This relative wealth is defined by the greater number of fine and exotic goods (ivory, metal, etc.) found in elite burials associated with permanent, often fortified settlements. Immediately to the west in well-watered central Andalusia, a region whose cultural sequence is now well understood (Arribas 1976), metal and other luxury goods are scarce and wealth differentials between burials relatively small; indeed, collective burial rituals persist well into the 2d millennium B.C., long after their replacement by individualizing burial rites in the arid regions to the east. Apparently, in better-watered regions dry farming was easier, capital-intensification of subsistence (in the form of irrigation) was less necessary, and, as a consequence, tendencies towards social stratification were less marked. Further instances of the regional association of social complexity with capital-intensive subsistence remain, of course, to be developed.

I have sought to put forward an account of the emergence of elites in Bronze Age Europe which will improve on the functionalism current among Europeanist prehistorians. The theory presented here is more faithful to the sequence and regional distribution of economic and social events in later prehistoric Europe. Thus, even in Greece and the Aegean, where the Minoan and Mycenaean palaces give managerial theories some plausibility, evidence for stratification precedes the development of centers for higher-order regulation by several centuries. In addition, a nonfunctionalist approach explains better the militarism which characterizes the accoutrements of Bronze Age elites throughout Europe. As the "protectors" established and consolidated their power over the capital-intensive food producers under them, means to display their superiority became necessary. The development of specialized technologies, such as metallurgy, and the trade in luxuries should be viewed as indications rather than as causes of the emergence of stratification. As Lancaster (1979) points out, in societies with capital-intensive agriculture subsistence and prestige are integrated into a unitary political economy of power. Finally, the outlook taken here corresponds better than the functionalist view to the actual role of elites in historically and ethnographically documented class societies. A focus on exploitation, rather than on management, as the central "function" of the ruling class constitutes a more uniformitarian view of social process in stratified societies. For this reason, the theory put forward here may help explain the beginnings of social stratification in other instances besides that of later prehistoric Europe.

Comments

by ROBERT MCC. ADAMS

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Gilman's analysis, while primarily concerned with prehistoric Europe, is of much wider interest and relevance. I generally share his view that explanations for the rise of social elites have too frequently stressed their integrative, managerial functions at the expense of an at least equally significant amassing of wealth and labor power for their own immediate benefit through conflict and other exploitative means. However, he seems to find it clear, while I do not, that there are coherent, opposed clusterings of functionalist and nonfunctionalist scholarly positions. It is even less apparent to me that we would score a conceptual or interpretive advance by wholly denying the former and insisting that only the nonfunctionalist school can contribute to an understanding of social evolutionary processes. As the increasingly fruitless controversies over new versus old archaeology and substantivist versus formalist economic anthropology also suggest, our aim should be to redress a balance rather than to prolong an oscillation between polarized, ideologically pure positions.

Ancient Near Eastern data shed some comparative light on other aspects of his argument in which a slight reformulation might be helpful. Particularly important is the place he assigns to craftsmanship, and especially metallurgy. In denying a central role for trade in the emergence of social stratification, he is at pains to identify the specialized production of copper and bronze as having been mainly for display, burial and votive hoard items having "a social and ideological rather than practical value." One may ask, however, to what extent the apparent lateness and rarity of archaeologically attested metal agricultural tools are sufficient demonstration of this point. Excavations that have been concentrated almost exclusively on graves and settlements are a grossly unrepresentative sample on which to base such a generalization, the more so since there is every reason to believe that broken items were valuable enough to be repeatedly reworked and recast. Gilman makes reference to clay sickles that were indeed ubiquitous in Mesopotamia during most of the 4th millennium, but it is equally significant that by the end of that span they had practically disappeared from contemporary use (Adams 1981), surely having been replaced by metal equivalents even though the latter are still almost unknown in archaeological context.

While welcoming the emphasis he gives to agricultural intensification as a stimulus to social stratification, I am similarly uneasy over his displacement of commodity exchange from any part in engendering this process. Granted that long-distance movement of foodstuffs was "quite beyond the capacity of Bronze Age transport systems" (save in cases like Egypt and Mesopotamia, where central riverine arteries permitted large-scale water-borne commerce), we cannot merely bifurcate cultural inventories into utiliarian products and luxuries and deny the importance of interregional trade on the grounds that it was confined to the necessarily restricted flow of the latter. A more functional view, but one not inconsistent also with Gilman's model of conflict-based evolution, instead might make a case for the social utility even of precious metals that were unambiguous luxuries. Naturally scarce, fungible, durable, and with a widespread reputation for fineness and integrity, they could be hoarded and rapidly deployed as a form of buffering against risk and uncertainty. The movement of gold and silver across much of the Old World continued steadily and with few interruptions, at least during all later premodern periods for which our knowledge is more adequate. For the Bronze Age, too, therefore, some form of luxury trade perhaps should be seen as a prototype for what later became "a disguised transfer of essential goods" (Schneider 1977:27) and "a major economic process—not merely an epiphenomenon" (Richards n.d.).

A final difficulty may be involved in Gilman's attempt to document the core process of the growth of social stratification from archaeological data alone. Rich female and subadult graves may indeed reflect "the progressive separation of high status from achievement" in a prehistoric European setting, but Mesopotamian cuneiform texts from the mid-3d millennium now are known that unambiguously attest to alternative possibilities. In one case, the purchaser of a field from the

eldest son of a dead temple administrator undertook to provide luxurious, carefully specified funerary offerings not only for the grave of the father but for the grave of the mother upon her eventual death (Steinkeller 1980). Such documentary evidence raises doubts that extend to areas like Europe where it does not exist. How genuinely separable are status and achievement anywhere, not for individuals but for social aggregates? By what analytical subtleties can we manage to interpret archaeological associations of grave goods so as to disentangle personal holdings, reciprocal transfers of material goods, and perhaps also gifts or exchanges intended to solidify various forms of alliance? The problem is compounded by numerous indications that the role of women, at any rate in early Sumerian society, was not the passive, subordinate one that is perhaps too quickly taken for granted for the Bronze Age generally. In short, increasing variability in the wealth disposed in graves could well have had a number of meanings for early Europe just as for Mesopotamia. We should acknowledge the considerable element of speculation in explaining such variability on the basis of movement along the single axis from modest status differentiation toward increasingly rigid and pronounced social stratification.

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The author's views on prehistoric economy seem to be based mainly on the categories and functioning of modern economic systems; it is at least questionable that we can legitimately use concepts as trade or capital investment with reference to Neolithic and Bronze Age Europe.

Furthermore, a basic cultural change (that is, a structural transformation such as the rise of social stratification) cannot be seen as determined by a single factor—here the presence of a "capital-intensive subsistence technology"—isolated from its context. In a general consideration of prehistoric Europe, it should be seen as the result of basically separate local developments associated with the existence of regional situations largely different in environmental, social, economic, and, more generally, cultural conditions. The emergence of social stratification should therefore be analyzed as a process internal to the regional cultural context and resulting from a whole complex of specifically local factors.

The author's criticism of the functionalist approach to the development of social stratification applies only to historical situations of advanced social division (Sumner and Rome are among the instances proposed), that is, to true class societies. In such instances the organizing activity of the ruling elites is obviously functional for the ruling class or classes and may or may not also be functional for the lower ones. However, this apparently does not apply to unstratified societies, in which specialized managerial or military activity of groups or individuals may well be functional for the community as a whole as well as representing the starting point for the formation of a hereditary elite (that is, for the rise of class division).

As regards "capital-intensive subsistence technologies," all subsistence techniques related to agriculture as the main economic basis of a society imply a substantial environmental change—precisely what the author would call a capital investment. Slash-and-burn agriculture in continental Europe under technical conditions of Neolithic type is a perfectly good instance. Plow agriculture and irrigation are widely diffused agricultural techniques with no special implications as "capital investments" in respect to others. Moreover, field divisions do not necessarily imply private ownership of the land, as is indicated by instances of the subdivision of communal land into individual family gardens or fields in modern "primitive" societies. Mediterranean polyculture is a complex subsistence technique that cannot be identified from the presence of olive pits or grape seeds alone, since it indeed implies permanent private ownership of land. In central Italy, this technique is not archaeologically known before the 7th century B.C., apparently as a consequence, not as a cause, of the emergence of permanent social stratification. Offshore fishing may be a "capital-intensive technology" (though not a durably effective one) but does not seem to be so clearly attested on archaeological grounds as to be considered a widely diffused subsistence technique in the Bronze Age.

The limited size of the European Neolithic and Bronze Age communities and the consistent discontinuity in settlement seem to exclude the hypothesis of durably successful "capital investment" and its effects as the basis for the emergence of permanent ruling classes during these periods. As a general hypothesis for prehistoric Europe, it seems possible to suggest that one or more of the preconditions for the emergence of permanent elites were present at different times and in different cultural areas of Europe at least from the Copper Age on, but the necessary combination of permanent preconditions (mainly, generalised labour division, settlement continuity, and large communities) did not appear before the Late Bronze and Early Iron Ages.

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Gilman has set himself the task of expressing in explicit terms the theoretical assumptions that form the basis of various interpretive hypotheses. Although I agree with many of his statements, there are some things that I find puzzling. The "functionalism" he discusses brings together scholars with vastly different ideas: Wittfogel, Service, Sahlins, Flannery, Renfrew, et al. Perhaps the one thing they have in common is the precedence they give to the social over the purely economic, which according to almost all of them derives from substantivist economics (in which case, Adams should also be included). The significance that social institutions assume, for example, in a neoevolutionist view cannot be applied to ideas based on systems theory. If it is right to criticize a harmonious conception of society and to emphasize the exploitative nature of the emerging elite, then it is necessary to point out that even Childe assigned such an elite an important function, which, independent of any moral considerations, contributed to the creation of a basis for more complex historical developments.

Explanations simply labeled "nonfunctionalist" run the risk of ending up as social psychological statements: Some individuals tend to dominate others and can only be stopped by the splitting up of the group; groups tend to split unless, for reasons of investment of capital, they are forced to confine themselves to specified territories. Gilman states that technological elements (the plow, vine and olive cultivation) are directly responsible for a closer connection with the territory occupied; these elements should, however, be considered not as causes, but as indications of an altered socioeconomic order. Whether or not one can really speak of a "ruling class" in the Bronze Age of Europe, class differences should be based on concrete divergences of group economic interests rather than those of individuals.

Although recent prehistoric research has recognized an independent development of the various European cultures during the Bronze Age, it would be wrong to exclude entirely the economic and social influences of highly complex societies deriving from the Aegean and the Near East. This would only serve to confuse the phenomena outlined by Childe with a generic "diffusionism." The evolution of social stratification during the Bronze Age of Europe is perhaps too complicated to be explained by a single mechanism of action and reaction.

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Gilman convincingly argues why Bronze Age European farmers preferred to stay in their villages rather than to migrate to escape the burden of exploiting (political) leaders. In this way—and in this way only—his well-documented article is a valuable contribution to our knowledge of the development of social stratification and political leadership. It does not explain, however, how and why stratification and leadership developed. We may safely assume that inequality and leadership already existed long before those farmers started to invest in their agricultural equipment. The new situation may have led only to a more complex or more intensive form of leadership and the formation of a clearly distinguished elite.

He rejects the view that the leaders or elite were benefactors of their peoples, and he is probably right in this. However, in doing this so rigorously he runs the risk of throwing away the baby with the bathwater. There is substantial evidence that sociopolitical leaders have served the interests of their peoples well and fairly. It can even be argued that the roots of leadership are found exactly in this quality (e.g., Lévi-Strauss 1967). That leaders were compensated for their activities seems reasonable and evident; reciprocity is an almost universal feature in human culture (cf. Sahlins 1965, Mauss 1970). That in the course of time, with the growth of sociopolitical complexity, reciprocal relations became asymmetrical cannot be denied (cf. Claessen 1978, Friedman 1979), but this is not sufficient to make a political elite exploiters *only*.

by GEORGE L. COWGILL
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I am strongly in sympathy with Gilman's general point of view, which is highly critical of functionalist explanations of the origins of social stratification and also of simplistic explanations of stratification as an adaptive response to environmental or demographic pressures. I believe Gilman has made a significant contribution to thought about social stratification in general, as well as to our understanding of Bronze Age Europe.

I have only a few suggestions for small improvements and clarifications. There is a slight tendency to speak as if beneficial management and selfish exploitation were mutually exclusive, although I doubt if Gilman really intends this. In any case, human affairs are more complicated, and many elites have surely operated by complex and shifting mixes of coercion, intimidation, bamboozlement, enthusiasm, and at least sometimes some real benefits for those beneath them.

Another source of confusion in functionalist arguments concerns the entity or entities being benefited. With reference to individual persons, the shift to stratification may mean a deterioration in the quality of life for all but a few. Yet, with reference to societies, a shift to stratification may be the only way to survive if the society finds itself in competition with other stratified and aggressive societies.

In connection with this last point, I wonder if bronze weapons were merely another means by which elites displayed their superiority or if they don't reflect a considerable increase in serious warfare as various elites competed with one another for significant material stakes.

Gilman makes good use of Earle's analysis of Hawaii and notes the relevance of ethnographic and historic studies of other more recent stratified societies. I strongly agree, and I believe that analysis, or reanalysis, of numerous other instances would support and refine Gilman's general thesis, if the work is undertaken without strong functionalist preconceptions and without the opposite preconception that all elites can be assumed to be purely and simply exploitative unless proven otherwise.

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Evidence for social stratification in the European Bronze Age is confined to burial furniture and a few isolated habitation sites. There is scant demographic information, and there are no data on settlement size, composition, or function(s). Nothing is known of the nature of social or economic relations or of land tenure. Lacking such information, it is particularly risky to speculate which segments of the community might manipulate capital and in what form capital might change hands.

Gilman rightly stresses the importance of new labor-intensive practices that undoubtedly concentrated capital in physical space, but he makes a leap of faith in the assumption that those who controlled fixed resources were the same individuals who were buried amidst *mobilier personnel*. The point is that with little evidence and no convincing ethnographic parallels, it is entirely possible that two segments of a Bronze Age community might have made rather different capital investments—one in heritable land, the other in easily disposed-of goods. In such a scenario, the dialectic of power between the two might have led in some cases to dominance by persons who controlled capital in both forms.

Gilman's argument that the individuals buried with precious goods were consistently the ruling class (i.e., at the top of a hierarchical social and economic structure) falls into the classic flawed functionalist category he decries: it is functionalist in that these people are mobile defenders of immobile agriculturalists, hierarchical in that they represent a group which controls the production of agriculturalists through tribute, and "evolutionary" in that complexity is again associated with hierarchical organization.

If one is to stress the independence of Europe from Oriental influences, then one must also stress the incredible variety of European cultures and their distinctive historic, economic, and cultural circumstances. A ruling elite may well have come into being in some areas when conquerors established dominance by force of arms over local agriculturalists. In other areas (as one sees in Africa, for example) a group of individuals may have established inheritable power in a few generations. In still others, the peculiarly European version of patronage, linked with the inheritance of particular parcels of land (mountain passes, river fords, etc.) suited for regional defense, may have solidified the social and economic position of a lineage. In some areas, it is possible that merchants and others who controlled a variety of information gained dominance through their association with more powerful contiguous groups.

In short, I would argue that no single theory of the origins of social stratification, functionalist or nonfunctionalist (is this a veiled reference to the New Structuralism? if so, Gilman should show the force of his convictions), can cover the multitude of cultural circumstances in Bronze Age Europe. Moreover, it is best to consider hierarchical notions of social stratification in Europe (for which only limited data exist) and elsewhere as a particular case of *heterarchical* (Crumley 1979) social structure. This general assumption would allow various factions—landed agriculturalists, religious figures, merchants, skilled tradespersons, and a host of others—to jockey (as they must have) for social, political, and economic position in a system open to both cultural (e.g., mercantile, military) and natural (e.g., climatic, topographic) variation.

by TIMOTHY EARLE
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Gilman's nonfunctionalist approach to explaining social stratification is most attractive, and I fundamentally agree with his argument. The functionalist position has had difficulty establishing how an institution with general survival value could be

selected for. In a small egalitarian society, the perceived advantage of an institution to the individuals of a group is sufficient to explain why the institution would have been adopted or elaborated. However, in a stratified society the situation is more complicated. As Gilman makes clear, because interests differ for the various classes within the society it is best to see how an institution functions to maintain a given class rather than the society as a whole. For example, irrigation, warfare, and trade may be selected for because they support elites and their control over wealth.

Thus we return to the question of how to explain social stratification. Can it be that elites are required to solve problems concerned with group survival? Gilman argues convincingly against this. Can there be technological or environmental characteristics that either provide inherent differential access to economic resources or, as Gilman suggests, enable control by elites because of the high costs of segmentation? This is the reasoning of the present paper. We must, however, still consider fully how elites provide services to their dependent populations and how this may affect the evolution of social stratification. Elites provide access to land and technology, some security against hard times, and protection from attack. Ethnographers frequently mention the paternalism of elites and the importance of this relationship to the peasants (cf. Johnson 1971). Although Gilman recognizes this, he emphasizes the way elites manipulate the relationship to increase their control of the commoners, and he dismisses the elite's functional role in the initial development of stratification.

Despite the attractiveness of this position, I feel that the functional characteristics of the elite for the broader society may be critical for the development and stability of a stratified society. In brief, whether a stratified system can develop may depend on the importance of the functions provided by the elite. During expansion, the elite must control new dependent producers as potential revenue sources. Critical to its ability to expand is the cost of controlling these added producers. A high-cost option is to control them through force. In most situations a lower-cost option is to provide critical services. In order to predict where stratification will evolve, it may be necessary to locate the conditions that minimize the cost of adding new dependent producers. Where the functions provided by the elite are of key importance, commoners are highly dependent on the elite and cost of control is relatively low. In other words, where management is advantageous to the commoner population, control costs are minimized and a stratified system should be able to develop rapidly. Such situations include those with frequent environmental and social disasters and those requiring complex subsistence technologies.

The potential success of a stratified system, therefore, depends on the cost of control. This in no way contradicts Gilman, but simply places his argument in a broader perspective. The inhibitors to segmentation that he discusses lower the cost of social control. Similarly, elites may be expected to alter economic relations in any way that will lower costs, for example, by restricting access to land. The ecological context and the potential functional importance of elites should be considered in this same perspective, for it may well be these factors that are crucial for the initial evolutionary success of stratification.

by ALAIN GALLAY

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Gilman's paper presents an extremely interesting approach to the origins of social stratification in Europe at the end of the Neolithic and the beginning of the Bronze Age. The method is a hypothetico-deductive one (Gardin 1974), which is unfamiliar to European prehistorians but is the only type of approach that can go beyond the dreadful historical explanations generated by an exclusively empirico-inductive method. My observations focus, on the one hand, on the characteristics of the model and, on the other, on its field of application.

1. *Characteristics of the model.* The model appears to have two weak points. The first concerns the problem of innovation in production techniques. The archaeological evidence on which the model is based is still very limited, except with regard to plow agriculture and the utilization of the ard. The development of Mediterranean polyculture, Iberian irrigation, and offshore fishing in the Atlantic and perhaps the Baltic requires more support. Furthermore, innovation is not, even from the author's point of view, explained, but only acknowledged.

The second weak point concerns the hereditary character of elites. The archaeological evidence supporting the hypothesis that elites were hereditary (rich graves of women and children) is substantial, and the interpretation of this phenomenon is interesting. However, the causes of the emergence of hereditary authority are not very clear. My experience of the ethnology of West African segmented societies allows me to suggest a solution to this problem. In a segmented society, lineage mobility is considerable. To the principle of segmentation is opposed the phenomenon of aggregation. The founding lineage of a village is joined by other families of varied origins, but it retains, through seniority, political power over the village (Gallay n.d.). This power remains limited as long as the chiefdom is transmitted within the lineage by age rank. Real and permanent lineage hierarchization becomes possible as soon as power is transmitted according to birthright (Meillassoux 1977). This mechanism, which is in the last analysis determined by seniority, could explain the development of a permanent lineage hierarchy.

2. *Fields of application.* In referring to several exotic societies, Gilman implies that his model can be generalized beyond Europe. In adopting this position, he runs the risk of admitting a simplistic evolutionism. As Scheurer (1979:120) has pointed out, this viewpoint is strictly conditioned by the advantage of hindsight, which privileges a single evolutive line and rules out all solutions which have had no historical descendants. In biology this was for a long time the position of the Darwinists and is currently in the process of being abandoned (notably with the concept of "évolution buissonnante"). It is necessary now to effect the same decentering of the observer in the historical disciplines. Only then will it be recognized that historical events are multiform and not entirely subject to the determinism implied by a dogmatic evolutionism.

The West African traditional societies give us, in this field, an example of a different evolution. In this region a segmented society of hoe agriculturalists coexists with a highly stratified society comprising chiefdoms, castes, and even slavery (Maquet 1967:224–27). This stratified society apparently has its origin in the development of trade in the Middle Ages, notably the trade in gold and salt (Mauny 1970). The structure and historical genesis of the society are therefore different here from the European situation proposed by Gilman.

by A. F. HARDING

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Gilman's article is an interesting contribution to Bronze Age research. It is well known that differentiation in both grave form and the provision of grave goods in Europe started on a big scale in the Early Bronze Age; this is interpreted, rightly or wrongly, as the start of marked social stratification. I am sure Gilman is right to be sceptical of the purely functionalist approach to this problem. Many of the lines of explanation advanced by the functionalists are simplistic and often seem to me to adopt an almost deterministic stance, concentrating on only one element in the complex mass of interwoven vari-

ables. At the same time, I am not convinced that the "nonfunctionalist" alternatives are not open to the same criticism. Gilman's professed aim is to "explain in nonmanagerial terms why societies with highly productive economies tend to have elites" and to suggest how elites establish and maintain their control "in spite of the fact that their actions do not serve common interests." After rejecting commodity exchange as an explanation, he goes on to consider four aspects of subsistence technology that he sees as representing "capital-intensification of subsistence." Although the precise social and economic mechanisms are different, the type of explanation provided by these factors seems to me much the same as the functionalist ones he has rejected. Gilman is right to draw attention to these factors, but whether they are really being used in a different explanatory way seems to me doubtful. A particular objection I would level at the model developed here is that it does not consider the very varied nature of wealth distribution in different spatial and temporal segments of the Bronze Age. What is right for, say, Branč in Early Bronze Age Slovakia cannot, surely, apply equally to Early Bronze Age Spain or Late Bronze Age Slovakia, let alone Late Bronze Age Spain, where different factors were undoubtedly coming into play.

One of the aspects of the study of social stratification which has been neglected in the past is the psychological one. What mental factors lead men first to desire and then to assume positions of dominance in given population groups? Concomitantly, what socioeconomic factors enable them to do it? All our explanations so far have been directed at the second part of the problem, none at the first. Naturally I cannot here go into this problem, even were I capable of it; I assume, however, that life in prehistoric Europe was, both economically and socially, competitive. At a crude level of description one can imagine men desiring increased status because they wish to be freed from the drudgery of subsistence labor, because they are naturally dominating by character, because they genuinely believe society stands to gain by granting them that status (in terms of protection against attack, production and distribution of goods, etc.), or for other similar reasons. Is it a natural and invariable aspect of human nature that some men will wish to achieve positions of dominance and others will be willing to be dominated (or at least unable to prevent it)? If it is, then the question we should be asking is why the Neolithic does not equally show social stratification. Could it be that the apparent indicators of status in the Bronze Age are not real indicators at all, but marks of fashion and individual preference? To return to the Neolithic: what material things changed between Neolithic and Bronze Age? Very few. At least some of the things Gilman lists, for example, plough agriculture, were present long before the Bronze Age, much earlier than the model predicts. Even metallurgy, which Renfrew and others have taken as one of the key stimulants to unequal distribution of wealth, was of course present on quite a large scale way back in the Neolithic, and I see no real reason that manipulation of water for irrigation or polyculture should not have been present too, even if we cannot yet prove it. To conclude: are we really sure that the supposed indicators of status in material culture are what they purport to be?

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By concentrating upon the processes of agricultural production and the opportunities they offer for exploitation, Gilman has much improved on the views Childe held 30 years ago. I think this paper provides a most interesting explanation for the rise of "High Barbarian" societies in Bronze Age Europe and strongly challenges some of the functionalist and palaeoeconomic interpretations that are now so fashionable.

It is no criticism to say that its chief weakness lies in the lack of empirical evidence for the capital-intensification of agriculture in more than a few scattered and patchy instances of unequal value. It is partly to remedy this that I am excavating a Bell Beaker settlement at Moncín, Borja (northern Spain), looking especially for traces of olive and vine cultivation. With A. J. Legge, I hope to be able to identify the points in the culture sequence of the Ebro Valley at which subsistence production changed gear, so that we can test Gilman's ideas. There may also be traces of early irrigation and specialized animal husbandry.

As an organizing model for the Bronze Age I find Gilman's ideas most attractive and quite as useful or convincing as the functionalist ones. Other pertinent work is that by Jodłowski (1976) on salt production in southern Poland around 3200 B.C. and my fresh synthesis of the whole of the Bell Beaker phenomenon (Harrison 1980).

by Ronald Hicks
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Gilman's thesis is a thought-provoking one. In particular, his paper is worthwhile for having looked at the problem of the development of the Bronze Age elites with the question "How *do* people behave?" in mind rather than just by manipulating "maybes." However, it raises, in my mind at least, a number of further questions that go unanswered.

For example, why, or how, did the elites begin to develop in first place? Gilman speaks of them as "protectors." If protection was necessary, why? If, as he says, plenty of land was still available, warfare provoked by population growth and competition for land does not seem likely. Were they simply gangs of thugs who instituted what amounted to a protection racket, threatening to do harm to any who didn't pay them? This also seems unlikely. Gilman claims that his argument is an alternative to functionalism, but it clearly would have been dysfunctional for the producers to abandon such resources as fields cleared for plowing, vineyards and orchards, or irrigated land. And if, as he implies, the elites were in some sense "protectors," this again involves a function, though not a managerial one. It appears to me that his thesis is therefore not an alternative to functionalism, but rather an alternative version.

That in some irrigating societies the elite has nothing to do with water distribution (he gives the example of medieval Valencia) doesn't mean this is always the case, and that a historical elite only affects distribution of a resource through managers or ceremonial functions does not mean that those elites did not originally have a more direct role. What about the evidence for theocratic and merchant elites? This needs to be explained somehow. What part *was* played by population growth?

It is easy to see why developed land would not be abandoned, but Gilman also mentions that offshore fishing with its heavy capital investment in larger boats and nets would have allowed the development of an elite. How? Why couldn't the fishers simply have sailed down the coast to another port if they didn't like the local elite? It isn't clear to me how this example fits into his argument. Could the fishers have been an important link in long-distance trade routes and unwilling to abandon that role?

In considering the origins of an elite, one must always look to small modern communities for examples. As Gilman points out, there are always aspirants to leadership roles; but they have to be provided with opportunities to assume such roles. There must be tasks—military, managerial, or whatever—that are necessary and that others in the society are less willing or able to assume. Simple inertia on the part of much of the populace seems to me to go a long way toward explaining the

persistence of elites. An individual, and by extension a family or line, gains power through exercising needed leadership. Through the reluctance of others to shoulder leadership responsibilities or to offend those who hold some degree of power, the elite is able to maintain itself and even gain more power. To the next generation it seems only natural that the elite should be there; if it continues to shoulder irksome responsibilities, it seems only reasonable that it should be allowed some tolerance in addition to material rewards. And so on. By calling our attention to the reluctance of people to abandon resources whose development has required a heavy investment of labor, Gilman has provided us with one more such reason for the maintenance of elites.

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This is an extremely valuable and important study on the beginnings of social stratification in Bronze Age Europe. Gilman's discussion of functionalist accounts which emphasize the essential and beneficial services provided by elites is cogent, convincing, and far less jargon-laden than other, deservedly well-known critiques (e.g., Friedman 1974). While the archaeological evidence supporting a *causal* connection between intensification of subsistence technologies and social stratification is tenuous or slightly ambiguous, the logic of his analysis that such intensification created a change in property relations and transferred "the problem of security from the material to the social field" is compelling. Although for purposes of discussion the remainder of my comments will be critical, I strongly believe that this is an exceptionally important examination of the emergence of social stratification; credible prehistory has been reconstructed which is consistent with our understanding of contemporary society and the exploitative role of elites within it. Functionalist accounts simply do not—to paraphrase Gilman—constitute a uniformitarian view of social process in stratified societies; they do not, in other words, pass the basic litmus test of reconstructing history as we know it from our daily lives.

My criticisms are both empirical and theoretical. Gilman takes as his unit of analysis the culturally and politically heterogeneous Bronze Age Europe. What possible justification can there be for jumping from Scandinavia to the Iberian peninsula, from Central Europe to the Aegean, other than the historical accident that Europe, a minor peninsula of the Eurasian landmass, constitutes a field of study, a specialty for Old World prehistorians? Europe or European civilization, of course, has meaning for the historian that relates to its shared religion and shared historical experience and that cannot be reduced to its physical characteristics; one must demonstrate, not assume, similar common features for prehistoric Europe. The point is important because the division between the Near East and Europe, the Orient and the Occident, is accepted as straightforward and nonproblematic. According to Gilman, *ex oriente lux* models have been empirically falsified; in my opinion, it is not proven, but highly dubious, that the Balkans, an area that we know was in contact with the Orient for millennia prior to the advent of metallurgy, somehow independently developed an extremely complex technology roughly at the same time that identical metallurgical techniques appeared in Anatolia, north of the Caucasus, on the Iranian plateau, in southern Turkmenia, etc. (Wertime 1973). Capital-intensification of subsistence technologies and consequent social stratification were more marked in the relatively arid regions of southern Europe, particularly the eastern Mediterranean, not for climatic reasons, but because these areas were involved in a larger historical reality (like the later Europe). This reality need not be conceptualized in classic diffusionary terms with the innovative Near East bestowing civilization (or here social stratification) upon barbarian Europe. Rather than dogmatically insisting upon isolated regional developments, we need to accept and modify Wallerstein's (1974:15) seminal concept of a "world economy" with interacting core and peripheral areas to understand the links between the Bronze Age Mediterranean world and continental Europe and the Near East. Societies from Central Asia to the Mediterranean were in contact with one another for the same exploitative, acquisitive reasons that led to the emergence of local elites. Cultures existed at different levels of development not simply because they had followed different evolutionary trajectories, but because more powerful societies could control and manipulate the level of development of their "Third World" neighbors. In short, we must be consistent in our uniformitarianism.

How we delimit our field of inquiry is not only an empirical, but also a theoretical question. Lurking behind or implicit in Gilman's analysis is a mechanical evolutionism that deserves careful scrutiny. Does the adoption of capital-intensive subsistence technologies inevitably result (sufficient condition) in social stratification or simply make possible (necessary condition) the emergence of elites? The distinction is crucial (Godelier 1972:274–75) and ultimately distinguishes a dialectical from a nondialectical view of history. Similarly, Gilman's programmatic assertion that for trade to have been important for the development of social stratification it had to have been essential begs the question of what is meant by "essential." Can essential items be socially or culturally defined, or must they be absolute and natural (i.e., subsistence-related)? Did the Neolithic farmers of southwestern Asia engage in the exchange of obsidian, sometimes on a substantial and significant scale (e.g., Jarmo, Tell Shemshara), because it was functionally superior to flint or other locally available chipped stone or because it possessed a culturally imposed value that cannot be completely understood in rational, utilitarian terms? Such a question is not meant to deny objective reality (clearly, only stones with specific physical properties could have been used in Neolithic times as harvesting tools), but to insist that a selection occurs on this reality. Acceptance of this fact does not imply a return to cultural particularism or the sterile, circular perspective that cultures vary because, in fact, they are different (cf. Harris's [1968:403] excellent objection to Benedict's patterns); the same attention to detail and critical awareness of people acting in their culturally perceived best interests that Gilman presents so tellingly in his attack on functionalism can be invoked to explain the creation and manipulation of less than objectively *essential* needs. Luxury metal artifacts may be both an index and a cause of incipient social stratification. Chiefs, undoubtedly, did not *need* many of the trinkets they used to separate themselves from the masses, but once they had come to expect them they willingly consolidated their power in order to continue receiving them. The substantial trade in textiles produced largely in workshops or factories in major urban centers that connected different regions throughout southwestern Asia in the 3d and 2d millennia relied not on recipients' physical need for clothing, but on artificially manipulated values that created a demand for fashionable, high-quality goods. It is clear, of course, that exchange in the contemporary Western world operates under a similar principle.

Undoubtedly, the intensification of subsistence technologies had profound social consequences. Gilman's focus upon this intensification and its effects is appropriate and directs our attention to basic considerations. His analysis provides the structure, not the narrative, for the emergence of elites in Bronze Age European societies.

by JAMES LEWTHWAITE
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Basically, Gilman deserves congratulations for his deflation of certain advocates of a benevolent squirearchy bent on agricultural improvement, a little modest trade, and the advancement of the deserving poor; it is all the more disappointing to find him thereafter content to confront the naive and simplistic with its own mirror image. His central proposition is that the 2d millennium revolved largely around an industrious but downtrodden peasantry unable to refuse protection money to a mob of flashily dressed racketeers for fear of having their plough oxen kneecapped, pirogues pirated, and olive trees set in cement overshoes. This *e mafia dux* historical melodrama is scarcely the "uniformitarian view of social process in stratified societies" its author considers it.

Gilman's problems stem from his aim of explaining the origins of what he terms "social stratification": this is too static and restricted a concept. To treat social contrasts as rigid divisions and concentrate solely on the exploitative aspect of "elites" is to narrow the scope of explanation to a self-fulfilling circularity. What is missing is the idea of the control of the reproduction of structures: the ethology of patronage, hegemonism, and clientage. Stratification is an anachronistic concept: it is the peculiarity of kin and client relationships that inequality does not precipitate alienation. The basis of the system is "from each according to his ability, to each according to his needs."

Both Renfrew and Gilman locate their social evolution in the superorganic. By contrast, Chagnon (1975) has brought social differentiation back to earth by asking how social preeminence, through differential success in life, enhances solidarity, thereby promoting the competitive chances of specific local populations in the real long-term struggle to reproduce themselves. From a Mediterranean perspective, clans, not classes, are the heart of the matter, even in the presence of ards, polyculture, irrigation, and seagoing boats. The clan, as a unit of analysis, must be the starting point for models of social differentiation. The widespread existence of modular biological, linguistic, and cultural units is the outcome of competing requirements for short-term subsistence and long-term reproduction. This is resolved by external appropriation and internal hierarchisation. Both intergroup warfare and intragroup competition favour the emergence of the patron ("big-man," "Godfather") and maintain his position. The competing clans assert their relative ranks in archaeologically visible form as conspicuous construction, distribution, and consumption.

Finally, Gilman, by denying the "elite" a managerial or redistributive role, reverses Renfrew's achievement of linking social and economic evolution, which forces him to resort to tired old "population pressure." An alternative model would see "intensification" as the progressive segregation and rationalisation of subsistence procurement in order to invest labour in higher-priority activities such as monument construction, the production of display items, and raiding, all of which affected, in the long term, group size and success in an autocatalytic development. The threshold of state formation occurs when the game passes from a situation where every participant works harder to stay in the same place to one in which the winner takes all.

In brief, each successive level of social differentiation, economic intensification, and modular growth represents an equilibrium achieved after a phase of intergroup competition.

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The assumption which underlies Gilman's thesis—that the development of social stratification during the Bronze Age was universal—is not demonstrated by the limited and varied geographical evidence presented. Different types of stratification may have existed in specific areas in response to varying social, economic, and political needs. His reference to fishing in Atlantic Europe, Scandinavia, and the Mediterranean is a good example. Certainly, this technology reflects social changes, but was the importance of fishing the same in each case? Also, does the existence of fishing imply the same developmental stage of social stratification, considering the time gaps between the different areas?

Although I do not discount the fact that there may be evidence for social differentiation in the archaeological record, I think that caution must be exercised in the interpretation of the data. Also, some information which might be useful to Gilman's argument has been ignored. For example, the work of Frost (1973), Betts (1973), and Schwartz (1976) on marine archaeology in the Aegean, establishing seafaring during the Neolithic and Bronze Ages, could have been used to support an argument for offshore fishing and trade. Further, in Eastern Europe there is evidence of site specialization during the late Neolithic based on the frequency of castrated cattle (Schwartz 1978).

Gilman's discounting of metallurgy as a possible cause for stratification precludes any association of it with subsistence strategies which likely preceded "capital-intensified subsistence." However, metallurgical technology appears to have been important to particular areas and settlements which were associated with subsistence agriculture (Jovanović 1979, Renfrew 1969). Gilman's idea is that "if such technologies are important to a group's subsistence, that group is dependent upon capital investments to which continued access must be insured by social means." Therefore, metallurgy could be a cause as well as an indication of stratification. In addition, the level of technology is likely to have varied among and within sites, as it did in western Hungary during the Middle Bronze Age (Choyke, personal communication, 1980).

Despite these criticisms, I consider Gilman's paper helpful in stimulating thought about stratification and its place during the Bronze Age. With a more thorough investigation from one geographic area, a likely hypothesis of how social stratification developed might be possible.

by STEPHEN J. SHENNAN
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Gilman has put forward an extremely important and interesting view of the development of social stratification in Bronze Age Europe, one which in its broad scope and ability to pick out important themes makes a pleasant change from the usual treatments of the period in terms of its bronze typology. He makes a good case for his "nonfunctionalist" viewpoint and for the importance of the factors he suggests in helping to explain the changes under discussion. However, within the context of a very positive view of the paper, I would like to make a number of specific criticisms, both of the situation reconstructed by Gilman and of his explanation for it.

There is certainly a great deal of evidence for social differentiation in the archaeological record of the European Bronze Age, but whether it is satisfactory to infer from this the presence of economically stratified class societies is another matter: many archaeologists would regard some if not all of them as ranked rather than stratified. This distinction is an important one, and much hangs on it in the context of Gilman's argument. It would have been helpful if it had been discussed in more detail and if the archaeological evidence for the stratification view had been more fully documented. This point raises itself in a number of guises. Throughout Gilman talks about elites, yet often this seems to be overstating the case: the Early Bronze Age cemetery at Branč, for example (Shennan 1975), indicates no more than a limited degree of rank differen-

tiation within the community, while to refer to bell beakers as an elite style gives completely the wrong impression. This may have been the case in Iberia, although Gilman later seems to refer to the late Copper Age cultures of Iberia as "ranked" rather than "stratified," but it is certainly not the case in the Central European part of the bell-beaker distribution area, where the Bell Beaker graves suggest no more than a minimal degree of ranking (Shennan 1977).

The insistence on the presence of elites seems in curious contrast to the denial of the existence of settlement hierarchies. Gilman seems to suggest that these imply managerial functions for the elite, but surely this is not the case. At the basis of any elite's power must be some form of regionally centralised control, a central person associated with a central place. As Gilman himself notes, the size of known Early Bronze Age settlements is minute, and inferences about population sizes from burial evidence also suggest that communities were extremely small. If Gilman is inferring a landscape in which there is simply an endless replication of individual communities, each with a leading family, to refer to those families as an elite seems again to overstate the case. In fact, there probably was just such a landscape in the Early Bronze Age Slovakia, exemplified by the Branč cemetery, but later on in the Bronze Age of this area and of others there seems to be quite good evidence for the existence of settlement hierarchies (e.g., Točik 1964), which would actually fit in better with Gilman's argument.

To move from Gilman's description of the situation to his explanation of it and his arguments that for trade to be important it must involve subsistence-related goods: I think he is essentially correct in his argument that bulk exchange of subsistence products cannot have been important in prehistoric Europe, although the possibilities offered by moving animals on the hoof should not be forgotten. However, in dismissing the importance of nonsubsistence trade he seems to make the same mistake as many of the functionalists he criticises. Friedman and Rowlands (1978), among others, have demonstrated clearly the possibilities for gaining power through the control of valuables essential for the transactions involved in the process of social reproduction. Simply to refer to such goods as luxuries is to neglect their potential social importance. In this context it is worth remarking Gilman's explanation for the greater wealth of northwestern Jutland revealed by Randsborg's (1974) study as the potential of adjacent fishing grounds; he neglects to observe that it is also one of the most prolific amber source areas.

Finally, one or two doubts arise from his discussion of the role of agricultural intensification in the development of stratification. First, so far as I understand the argument, he seems to be assigning an autonomy as independent variables to the plough, olive trees, and irrigation systems, and this seems to me rather dubious. Secondly, it seems to me a problem arises in his discussion of the chronological priority of intensification over stratification. There may well be a link here, but in Denmark, for example, the best part of a millennium elapses between the first appearance of evidence for ploughing and evidence for the emergence of social differentiation in the local Early Bronze Age. One would not wish to argue that there must always be a contemporary and correlating "cause" for every effect, but the gap, which is present in other areas as well, suggests that if intensification was necessary for the development of stratification in Bronze Age Europe it was certainly not sufficient.

by ANDREW SHERRATT
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Despite the fact that more is known of the prehistory of Europe than of that of any other region, discussions of the emergence of social stratification have largely concentrated on areas such as Mesopotamia and Mesoamerica. The long agricultural sequence before urbanisation in Europe poses problems because of the relatively late appearance of the conventional criteria of state organisation and the evanescent occurrence of features such as ceremonial monuments and defended centres. Yet clearly there are fundamental contrasts between the temperate cultivators of the "Neolithic" and the "Bronze Age" which go beyond the 19th-century technological criteria. It is all the more important, therefore, to avoid imposing inappropriate divisions on this long sequence. The stark contrast between "stratified" and "nonstratified" societies does not do justice to the nature of the problem, however relevant it may perhaps be to the appearance of powerful centralised states in southwestern Asia. The occurrence of occasional wealth objects in subadult graves and the general increase in status-linked manufactured products hardly add up to the kind of stark social contrasts implied by the term stratification. It is the restricted nature of elite differentiation, rather than its extent, that demands explanation in this context. Instead of a confrontation of rival paradigms, closer attention to the contrasts between different areas—notably between Mediterranean and temperate Europe—is the key to understanding the dynamics of this process.

Despite Gilman's failure to make use of the *variety* in the evidence at his disposal, he perceptively identifies some of the common factors leading towards social change which characterise the later phases of European agrarian prehistory. While it is unlikely that the earliest European agriculture consisted of slash-and-burn farming (and "flood-water farming" is likely to have been an original feature of Mediterranean cultivation rather than a Bronze Age innovation [Sherratt 1980*a*]), there is nevertheless a significant horizon of change in the mid-3d millennium, associated with the introduction of ox (not horse!) traction for ploughing. This fundamentally altered both the agrarian and the social basis of European communities, as I have tried to show (Sherratt 1980*b*). The increase in the scale of animal utilisation, associated with the keeping of sheep for wool and milk, is likely to have been especially relevant to social differentiation in the Mediterranean in the 2d millennium, through the differential accumulation of stock.

The question of tree crops is more problematic, and it is possible to argue that the extensive cultivation of vine and olive is as much a consequence of economic centralisation as a cause. The date of the introduction of tree crops in the western Mediterranean (including also the use of the chestnut) deserves more systematic discussion than it is given in this article. Fishing, on the other hand, is clearly basic to an understanding of Bronze Age economies in both the Mediterranean and the Baltic, and the importance of maritime trade is reflected both in artefact distributions and in coastal locations for settlement.

On the wider question of the role of elites, it seems unlikely that the oversimple view of their essentially parasitic existence is any more valid than the converse view which sees them as benevolent providers of exotic necessities. As an antidote to the Panglossian ecological interpretations of the 1960s, however, Gilman's paper offers not only a useful antithesis but one which accords more closely with the world after the oil crisis.

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The essence of Gilman's contribution is the effort to develop a systematic relation between economic facts and forms of social complexity. Since archaeological data store extensive information on economic activity, grounding a set of propositions for

the systematic explanation of social evolution in economic activities may ensure them greater validity for theory building. Having been working along similar lines myself, I believe that, to be properly appreciated, beyond the limitations imposed by its relative brevity and certain contradictions due mostly to the random selection of examples, the paper should be positioned within the ongoing controversy on the evolution of complex societies.

Functionalist theory is based on ethnographic data, which allow a broad spectrum of observations and an interplay between material facts and the accompanying ideological background; naturally, superstructural aspects become dominant and the stress is on cultural and individual diversity. Archaeological data are very different, but their presumed inferiority is essentially based on the assumption that they should be demonstrative of a theoretical corpus generated from another universe. We might accept that superstructural dimensions dominate infrastructural ones, but there is no reason to take for granted that a whole class of data, the only real information we have on extinct populations and the primary stages of social evolution, is deficient.

Like Spriggs's (1977) attempt to reopen the discussion of the correlability of archaeological and ethnographic data, Gilman's article is evidence of an uneasy feeling that is spreading among archaeologists who believe in the historical dimension of human evolution. Central to this discussion is the notion of economic growth as expressed by the accumulation of wealth directly related to the division of labour (horizontal inequality) and to the hierarchical/vertical inequality in access to resources and investments. The neglect of this concept in recent years has undoubtedly contributed to the success of the superstructural perspective in research on the evolution of complex society. "Economic growth" became obsolete at the same time as "surplus," in the late 1950s, with the success of substantivism and of the functionalist-structuralist offensive against Marxism. The devaluation of archaeological data was largely a by-product of this conceptual settlement, since the common feature of these various schools was that the evolution of man, being essentially superstructural, is largely nonlinear and therefore ahistorical. The political dimension of this selection becomes more apparent through a critical look at the state of prehistoric research today.

Nowhere in the world is prehistoric archaeology institutionally independent. With few exceptions, it is subordinate to either the social or the historical sciences. The subordination is explained in terms of the shortcomings of archaeological data in comparison with those of either anthropology or history. Childe (1946) attempted to overcome this barrier by suggesting that it was possible to sketch human evolution in such a way as to make it testable archaeologically. His attempt was largely unsuccessful in his time. Meanwhile, techniques were developed that gave prehistoric archaeology the means to become a source of coherent information on early societies: stratigraphic excavations, total recovery, settlement studies, ecofact-artefact correlations, quantitative analysis, and, of course, the multidisciplinary methodology that integrated the natural sciences into the study of man's recent past. Childe's ideological opponents, among them Clark and Wheeler, mastered these innovative methods and readily demonstrated the factual unreliability of his propositions, which were labelled "politically motivated" (Clark 1976). As a result, the study of complex societies has remained firmly in the hands of cultural anthropologists. In continental Europe prehistoric studies have been dominated by the Montelius/Müller-Karpe continuum that for a century has been the backbone of what we may call descriptive formalism. In this rigid system, based on the total recording of the artefactual evidence, there is no room for theory building, which remains the preserve of historians.

The "New Archaeology" aggregated in a single coherent discourse all the various methods that had been developed by the previous generation (particularly the environmental dimension in regional studies) and imposed hypothetico-deductive modes of investigation. This approach has been largely confined to the areas in which it developed, west of the Appalachians. Notwithstanding the important work it has produced, not a single representative of the historical core of the movement has occupied a major position in one of the great universities of the East Coast. In England the reaction was outspoken, coalescing in the 1971 Sheffield symposium organized by Renfrew (1973*b*). Although they were not directly proposing a neo-Childean or materialistic approach, the "New Archaeologists" gave too much attention to aspects of material culture in the understanding of complex societies not to stimulate a strong reaction. Since 1971 Renfrew has been active in developing this perspective of social evolution as dominated by noneconomic factors. The "New Archaeology" has had little impact elsewhere except in Scandinavia, where we find groups clustered around the *Norwegian Archaeological Review* and C.-A. Moberg in Göteborg. Basically its propositions have been either ignored or confuted on epistemological grounds by the neopositivist criticism of Gardin (1980).

In my opinion Gilman's paper should be assessed in the light of the theoretical paralysis confronted by prehistorians since the "New Archaeology" began retreating under the onslaught of functionalism, on the one hand, and taxo-formalism, on the other. I see it as a kind of cornerstone of the theoretical structure that will house a post-Childean materialistic archaeology. This orientation is beginning to appear in a very scattered group of scholars evenly spread worldwide. They still have very little in common apart from a certain uneasiness about subordinating the rich bodies of data they control to a theory generated from evidence they cannot control. Gilman calls this the "nonfunctionalist alternative" and see its manifestation in emphasizing the reconstruction of modes of subsistence in order to isolate the capital-intensive processes crucial to the growth of wealth in a given area and period. The priority of this perspective is founded on the fact that the means of production and the organizations of labour necessary to the development of complex societies almost everywhere pre-date the emergence of social segmentation, thus suggesting that the demand does not create the supply, but selects among existing options. In more general terms, we might outline this type of approach as the search for causative patterns linking natural conditions and social structures through detailed analysis of economic activity. Economy is the social and technological context of the transformation of nature by human activity. "Nature" here represents the whole of Earth's resources exploited by a given people in a given period. These resources must be identified and correlated in various directions to provide the framework for the reconstruction of the particular economy. This is largely what Gilman has done here. We need more regional projects, each involving a detailed reconstruction of modes of subsistence in their evolution from the adaptive stage of the Late Stone Age to the nonadaptive surplus-producing phases that immediately precede social stratification in early states. I stress the regional dimension because it is the only possible way of understanding economic structuring in so diverse a territory as Europe. In peninsular Italy, for example, it was not irrigation or plow agriculture that may have been determinant of the expansion of means of production, but what I have called the "conquest of the hills." Territorial expansion was possible only if farming could be carried out on slopes, for plains represent less than 10% of the land area. This process was almost complete by the end of the Bronze Age and was probably accomplished in two stages: first increasing terracing of the hills and then the introduction of crops adapted to the slopes. These crops, primarily fruit trees and vines, have become the main feature of European farming, and Gilman is right in pointing to their importance. I would emphasize that to a great extent the individual factors of capital-intensification of

subsistence activity might be grouped together as interrelated aspects of the same process.

Appropriate use of archaeological data in this materialistic perspective requires the articulation of the economic reality in concepts suitable to this kind of evidence and at the same time the linking of it to relevant social institutions, loosely defined in terms such as "rank society," in order to identify activities and products classified in terms of their capacity to extract, transform, and store resources as well as to generate growth and surplus (Tosi n.d.).

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Gilman's paper is an interesting and thoughtful approach to the problem of origins of social stratification. The discipline of European prehistory has suffered from the lack of attempts to explain important changes evident in the archaeological record, and this essay is a welcome step in that direction. I wish to make one general suggestion, then comment on two specific points.

It would be helpful if the author would define several terms whose meanings are understood in a general way but whose specific connotations can be important. These include "functionalist," "elite," and "ruling class."

My specific remarks concern the applicability of the model to central Europe. Of the four aspects of "capital-intensification" of subsistence, only plow agriculture could possibly apply to central Europe. Several recent studies suggest that the major investment of time and energy in the preparation of land for farming occurred much earlier than Gilman indicates, beginning with the start of the Neolithic. It appears now that the first farmers of central Europe did not practice slash-and-burn agriculture, which is unnecessary in the rich soils of the area (Modderman 1971; Jarman 1976: esp. 137–40). The evidence suggests a permanence of occupation of settlements from this earliest phase, with attendant clearing of fields for long-term agricultural use. Yet clear evidence of social stratification does not appear until much later.

In arguing against the role of the development of bronze metallurgy in the formation of elites, Gilman suggests that very few agricultural implements of bronze are known before Late Bronze Age times. In central Europe this is not the case. The sickle, the most obvious agricultural tool of metal at the time, is well represented in settlements (e.g., Fischer 1971:13 and pl. 3,5.6) and in hoards. The reasons that no more are known from settlements are twofold. First, very few Early and Middle Bronze Age settlements have been excavated in central Europe. Second, usable sickles would not ordinarily have been left behind in abandoned settlements. Broken sickles would have been lost in the fields or thrown back into the metalcaster's pot.

A substantial number of hoards of Early and early Middle Bronze Age date contain sickles. Some contain new, unused objects (e.g., Krahe 1963) and can be interpreted as caches of new tools deposited by traveling merchants or metalsmiths for future, unrealized recovery. Others contain primarily broken objects and scraps of bronze (e.g., Dehn 1952, Kimmig 1955) and probably represent collections of metal destined for remelting and recasting. There is no need to interpret either kind of hoard as "votive," and both represent aspects of the rapidly growing industry and trade in bronze implements at the beginning of the Bronze Age (see Reinecke 1930, Pittioni 1976).

The evidence of bronze tools suggests to me that this metal was playing a role in agricultural production from the Early Bronze Age on in central Europe and that the management and control of trade in bronze may have been a significant factor in the emergence of elites during this period.

Reply

by ANTONIO GILMAN
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Social evolutionists have had difficulty in formulating an adequate account of the transition from classless to stratified societies. On the one hand, the logic of their endeavor constrains them to give causal primacy to technological changes, especially as these affect subsistence. On the other hand, in the complex societies which emerge from the transition social factors clearly have causal primacy in the dynamics of culture change: where social positions are determined not only by age, sex, and achievement, but also by birth, a class analysis is essential to the understanding of history. Most evolutionists have attempted to reconcile technological and social causation by suggesting that the elites of early stratified societies arose as a result of the managerial, organizational requirements of more powerful methods of production. I have tried to show that this formulation is unsatisfactory and to suggest an alternative way of reconciling technological and social causation: the autonomous development of capital-intensive systems of production by household groups opens up the opportunity for a minority to attain permanent superordinate statuses by exploitative means. It is gratifying to see that almost all of the commentators are generous enough to find my approach of some interest.

It is impossible, of course, to do justice to the variety of the comments in any reasonable space. I will attempt to respond to the several empirical and theoretical questions which are shared by a number of reviewers. Some criticisms (such as Lewthwaite's belief that I consider "tired old 'population pressure' " to be a prime mover of social change) seem to be based on misunderstandings (or possibly misreadings) of what I have written. Where I do not discuss points raised in particular comments, the reader must judge for himself how to resolve whatever disagreements may exist. The argumentative content which the format of this reply entails only reflects my appreciation of the help of my colleagues in clarifying my thinking on issues of mutual concern.

Problems of evidence. For prehistorians paucity of data is an inescapable fact of life. For the problems I am dealing with, the general inadequacy of archaeological evidence as such is exacerbated by the small number of studies which focus on economic and social questions. I am particularly grateful, therefore, to those colleagues (Harrison, Schwartz, Shennan, Sherratt, Tosi, and Wells) who have confronted my thesis with concrete evidence. I will turn to some of their concerns below.

As Adams, Crumley, Harding, Shennan, and Sherratt point out, I move perhaps too quickly over the crucial (and inevitably thorny) area of burial sociology. Indeed, it is fair to say that I accept a consensus which suits me rather than subject it to detailed criticism. I agree with Shennan and Sherratt that the wealth differentials seen within Bronze Age cemeteries often are not large and with Adams that the key question of whether the contrasts reflect achieved or ascribed status differences can never be conclusively settled using purely archaeological evidence. Nevertheless, there are striking contrasts between Neolithic and Bronze Age burial rites, and the prima facie interpretation of these divergences as reflecting the presence of hereditary status differences in the latter tends to be confirmed by more detailed analysis. A diligent search through the ethnographic literature no doubt would find (and it would be even easier to imagine) "archaeological nightmares" (Ucko 1969) in which comparable contrasts would merely reflect a slight increase in ranking (or have no social significance at all). On the whole, however, I find the consensus view acceptable, and so, for that matter, do those who have commented here (the

reader should consider, for example, the passage from Coles and Harding's [1979] survey quoted above). To say that more work needs to be done is not to say that the most likely conclusions based on the evidence now available are incorrect or should be ignored.

Even more tenuous is the evidence on land tenure, which Crumley correctly sees as a crucial question. The surviving "Celtic" field systems suggest, however, some stability in landholding. (As Bietti Sestieri should know, it is not the mere existence of boundaries which leads to this interpretation, but the way in which the boundaries are formed: the lynchets separating the fields are created by plowing's acceleration of soil creep; the existence of a field system delimited by lynchets implies, therefore, that the same fields were plowed in the same configuration for a long time; this in turn suggests an orderly regulation of access to the land, in other words, some form of ownership.) Stable landholding patterns imply a commitment to fixed resources which would permit the development of a protection/extortion complex. While I obviously cannot specify which fields provided the surplus that generated the wealth in which graves, it hardly seems a "leap of faith," as Crumley describes it, to suppose that, taken as a whole, the wealth of Bronze Age elites represents a share of subsistence production. In Classical times, when society was certainly much more complex and "heterarchical" than during the Bronze Age, the fundamental source of wealth was, as Finley (1973) stresses, the land. It seems a matter not of faith but of reason to suppose that in the simpler societies of European barbarians the land and those who worked it would also have provided the surplus captured by the privileged few.

It is easy to be critical of archaeological data, to denounce conclusions as speculative, to indulge in *pro forma* methodological pessimism. Most prehistorians are selective in their conservatism, however. When the evidence leads them to conclusions they like, the soberest scholars speculate. Thus, Bietti Sestieri, who here considers it "at least questionable that we can legitimately use concepts such as trade . . . with reference to Neolithic and Bronze Age Europe," has elsewhere averred, on the basis of typological parallels alone, that in the later 2d millennium B.C. Mycenaean smiths were working in Italian metal workshops (Bietti Sestieri 1973:408). Crumley, who considers it "risky" to suppose that the occupants of *Fürstengräber* obtained their wealth from agricultural producers in their vicinity, is willing three paragraphs later to speculate on the existence of merchant elites and on toll-collecting at fords and passes in Bronze Age times. No archaeologist can afford to wait until there is enough evidence to make the writing of prehistory "safe." What we can do is argue our cases for what the past was like from the central tendencies of the available evidence and with a realistic grasp of historical process.

Several commentators (Bietti Sestieri, Cazzella, Crumley, Harding, Kohl, Schwartz, and Tosi) suggest that developments in Europe during the 3d and 2d millennia B.C. may be too diverse for any single theory to account for them. I am not attempting to explain all aspects of Bronze Age diversity, but within the scope of my interests I disagree with this criticism for three reasons. In the first place, the model I put forward is a structural one and thus accommodates empirical diversity in specific cultural details. I suggest a relationship between capital-intensification of subsistence and exploitation which can be applied to a variety of specific modes both of intensification and of surplus capture. The model is, so to speak, processual and thus can account for several European transitions from ranking to stratification. To put the same point another way, if, as Adams and Cowgill seem to feel, my approach deserves consideration in entirely non-European contexts, then it may also be relevant to several different areas within Europe. In the second place, the model accommodates structural diversity. I do not assume, as Schwartz seems to think, that social stratification was universal within Europe. On the contrary, I acknowledge the differential rate of social change and attempt to explain this by the differential rate of capital-intensification. If anything, Sherratt's criticism of my "failure to make use of the *variety* in the evidence" is more just than the view that I cast too wide a net. In the third place, there are, in fact, strikingly similar developments in many areas of Europe during the Early Bronze Age. As Shennan (1980) stresses, the general character of elite burials and many of the specific artifact types included in them (e.g., rivetted daggers) are much the same in southeastern Spain, Brittany, Wessex, Saxo-Thuringia, Bohemia, and so on. This elite complex appears in these different areas at about the same time (the beginning of the 2d millennium) and contrasts with the much less differentiated burial rites of the preceding period. Given these broad similarities, it seems reasonable to me to suppose that one can attempt a common explanation.

Harding, Shennan, and Wells are concerned about the time lag between the introduction of subsistence intensification and the development of hereditary inequalities. In a basic sense, of course, some capital stocks are inherent in the simplest food production systems: in one form or another, farmers will have stores to ensure future production, tide them over bad harvests and lean seasons, and so on. These stores, being of general value, must be defended, and settlement fortifications, nonexistent in the Palaeolithic, become frequent in the Neolithic. It is the general understanding of social evolutionists that ranking arises in response to the need to create and defend these stores (Service 1962, Fried 1967). The question is, therefore, at what point the community's assets become significant enough to permit the shift from ranking to stratification. Clearly, the relatively unintensive agricultural practices which, *pace* Wells and Sherratt (1980*a*), characterized the earlier Neolithic in Europe would not have entailed such critical accumulations of capital. Clearly, too, the first introduction of more intensive practices would not immediately have provided big-men the leverage necessary to become chiefs. We need many more detailed regional assessments, such as that provided by Bradley (1978) for the British Isles, of the complex environmental and technological factors involved. In the absence of such studies, I can only agree with the inevitably impressionistic assessment of Sherratt that there is a "significant horizon of change in the mid-3d millennium B.C." The adoption of the plow and other intensifications would not immediately have led to stratification, but it is apparent that the older social order was not unaffected. The replacement of collective burial in megaliths by single grave rites in northern Europe, on the one hand, and the intensification of collective rituals (in the face of the undermining of their material base) in Wessex (the construction of large ceremonial monuments by the "group-oriented chiefdoms" of Renfrew [1974]), on the Boyne (the spectacular New Grange passage-grave group), and in southeastern Spain (the Los Millares phenomenon), on the other hand, may be interpreted as varying responses to the stresses which the capital-intensification of agriculture produced in the Neolithic social order (Gilman 1976, Shennan 1980). I think it is perfectly reasonable to suppose that it would take half a millennium, or even longer, for these stresses to be resolved within a new social order.

Commodity exchange and bronze. Adams, Kohl, and Shennan believe that I underestimate the importance of primitive valuables in contributing to the development of stratification. I agree with Adams that the "naturally scarce, fungible, durable" properties of bronze would make it an ideal medium for storing and mobilizing capital. Bronze and other such preciosities would thus help the elites that possessed them to consolidate, extend, and transmit their power. One can also see how preferential access to a valuable prized in the core of a stratified system could give rise to compradore elites along the system's periphery. This was Childe's view of Bronze Age developments in Europe as a whole, of course. Shennan follows

a similar line on a more local scale when he argues here that amber may have been important to the beginning of the Danish Bronze Age: since the first horizon of clear stratification in the North occurs centuries after classes had emerged in Central Europe, the former could have played periphery to the latter's core. All these approaches assume, however, the prior existence of the capital which the preciosities will represent. When we are dealing with the pristine development of hereditary elites, the key question is how they captured surplus, not into what convenient form they converted it. Metallurgy was known for over a millennium in Europe before the intensification of subsistence systems created a social context for the storage of wealth and stimulated the florescence of the technology. It is difficult to accept that preciosities could have played more than an ancillary role in an autonomous social evolution towards stratification. To argue otherwise would amount to saying that a fundamental change in human social systems "began with a caprice" (Schneider 1977:23).

My opinion that the development of the Bronze Age in Europe was essentially an autochthonous process is based not on dogma (as Kohl seems to think), but on evidence. If the Aegean in the mid-3d millennium or El Argar, Aunjetitz, or Wessex in the early 2d millennium had been peripherally involved in a Greater Near Eastern "world system," one would expect this to be reflected materially by the presence of certifiable imports from the putative core areas. In spite of the fact that such finds were predicted by the theory dominant in prehistoric European studies for almost half a century, there are none in Western and Central Europe earlier than the 1st millennium and extremely few in the Aegean earlier than the 2d (Renfrew 1972:211–17). Cazzella may wish not "to exclude entirely the economic and social influences" of the Orient and the Aegean, and Kohl may insist on principle that an analogue to Wallerstein's (1974) theory of the development of capitalism is applicable to prehistoric Europe, but I fail to see how these connections can be both economically and socially important and materially invisible. Connections extensive enough to effect significant social change over wide areas of Europe must be expected to involve at least some artifactual consequences. The core/periphery model may help us understand the Aegean Late Bronze Age or the Central European Early Iron Age, but, when social stratification first arose, Europe was, in Wallerstein's terms, marginal.

Whether or not core and peripheral areas can be distinguished *within* Bronze Age Europe deserves detailed empirical examination. I am not persuaded, however, by Shennan's suggestion about the causal importance of amber for the development of stratification in Denmark. Randsborg (1974) shows a general correlation between agricultural productivity (based on the plow) and degree of inequality. If Shennan were right, one would expect the entire west coast of Jutland to be an exception, since it is uniformly a source of amber and relatively unproductive agriculturally. Only the area around the Limfjord, a rich and sheltered fishing ground, fails to fit Randsborg's correlation. The facts, such as they are, fit my speculation better than Shennan's. The idea that in some areas of Europe secondary elites arose as a result of their compradore status in exchanges with more powerful elites elsewhere within the continent merits careful consideration, but it is hard to reconcile with the generally rudimentary character of European Bronze Age stratification (which Shennan himself underlines in his comment here).

A commodity-exchange theory of the origins of social stratification applicable to the European Bronze Age must point to internal or external commerce in goods which meet the basic needs of households. To the extent that households could do without the goods, they could do without the exactions of the suppliers. Evolutionary logic suggests that we look to the subsistence sector for such commodities, yet it is apparent (and no response disagrees) that significant trade in foodstuffs is unlikely to have existed in prehistoric Europe. A remaining possibility for those who would defend the importance of trade in the development of social inequalities in Europe is to argue that bronze was important in increasing agricultural production. This is the position of Schwartz and Wells. Schwartz observes that centers of early metallurgy are associated with areas of important subsistence production. This proves that miners and smiths must eat, not that metal tools were important in agriculture. Wells indicates that at the start of the Middle Bronze Age (Reinecke A2/B1) in Central Europe agricultural implements of bronze are found in settlement debris and foundry hoards. Excluding casting fragments and counting each piece as a whole artifact, the composition of the metal assemblages he cites is given in table 1. Arbon-Bleiche is a settlement site, and, as Wells indicates, taphonomic considerations are sufficient to account for the relatively small number of sickles and large number of awls, needles, etc. ("Other Artifacts"). The other two assemblages are foundry hoards consisting mainly of casting fragments and broken artifacts; these should be fairly representative of the metal in use. It is clear that one must interpret axes as agricultural implements (presumably used for land clearance) for any significant proportion of the metal in these assemblages to be considered relevant to food production. Because they are found in graves with swords and daggers, axes are usually interpreted as weapons. In the absence of analyses of the use marks on axes and of experiments determining the effectiveness of replicas in wood chopping (see Coles 1979:101–4), the issue is hard to resolve. Experimental evidence does show, however, that bronze sickles are not much better than flint ones in terms of harvesting efficiency (Coles 1979:117–18). As usual in archaeology, the issue is not clear-cut, but the evidence Wells presents does not persuade me that metal artifacts were as important technoenvironmentally as they were socially and ideologically.

I do not think that the development of exchange systems lies at the root of the emergence of a hereditary elite in Bronze Age Europe, but this does not mean that such a process could not account for such developments in other historical or ecological settings. In other instances, where ecological conditions favor specialization in subsistence or require importation of commodities needed to maintain secure production or where a core/periphery trading system can be documented, an exchange-based theory may well be viable. All the same, the "decentering of the observer" which Gallay recommends should not imply that essential processes will differ in separate areas. Any account of the origins of stratification must explain in terms of the survival strategies of household productive units what constrained the mass of the population to accept the ascriptive component of superordinate status.

Functionalism and the role of elites. Crumley, Harding, and Hicks seem to feel that my approach is, after all, a functionalist one. To the extent that I do not believe society to be a thing of shreds and patches, I of course must be a functionalist. As I attempted to explain at the start of the "Critique" section of

TABLE 1

COMPOSITION OF METAL ASSEMBLAGES FROM THREE CENTRAL EUROPEAN SITES

SITE AND SOURCE	SICKLES	AXES	OTHER WEAPONS	ORNAMENTS	OTHER ARTIFACTS
Arbon-Bleiche (Fischer 1971).....	2	4	16	35	42
Böhl (Dehn 1952).....	17	9	8	230	1
Ackenbach (Kimmig 1955)....	12	9	10	22	–

my paper, however, a belief that social systems are integrated need not involve the view that a particular aspect of a society (in this instance, hereditary preferential access to resources by a ruling minority) is necessary to that society's (and its participants') existence. This, however, is precisely the argument of the functionalists-with-respect-to-stratification: the leadership of an elite, they say, is necessary to achieve the Darwinian good of higher and more secure production. My position is that certain efforts by households to achieve higher and more secure production provide leaders holding their positions by their achievements the leverage with which to make their positions permanent. By adding the threat of violence against the now immobile mass of the population to the promises of assistance they gave before, the leaders could make their status hereditary and reduce the amount of assistance to their followers. My approach may be functionalist in the broad sense of recognizing the interdependence of the social and the technological, but it is nonfunctionalist with respect to the relationship between stratification and production.

I agree with Cazzella, Claessen, Cowgill, Earle, Lewthwaite, and Sherratt that (to repeat myself) "it is undeniable that ruling classes may sometimes be of service to their subjects." In order to maintain hereditary power, an elite must use both the carrot and the stick. As the collapses of the Pahlevi and Somoza dynasties show, even in our own day, when the technology of force is incomparably more powerful than in the past, it is impossible for even the most ruthless ruler to maintain his power unless he reconciles an adequate proportion of his subjects to his regime by means of positive incentives. It is important to be a "good massa." At the same time, no stratified regime is content to confide the position of the elite to the consent of the governed: force is the ultimate guarantee of power. As Earle points out, the extent to which elites extend their power and the ease with which they manage their subjects are largely determined by the judicious provision of services to at least some of the population. In the final analysis, however, an elite maintains its power through its predictable ability to apply violence effectively to all its subjects.

I emphasize the role of negative incentives in the emergence of hereditary elites because precisely what is characteristic of the transition to stratification is the normally successful use of force as one of the means by which leaders maintain their power. Where systems of production are unintensive, the threat of violence is mostly ineffective: the population can abandon its would-be master. Where systems of production require important capital investments, the population cannot escape the unwanted attentions of its leaders. During the millennia which preceded the development of capital-intensive subsistence, all the services provided by egalitarian redistributors failed to gain them hereditary leadership positions. Once the successful application of force became possible, stratification emerged within a few centuries.

Lewthwaite's amusing comments concerning my Mafia theory of historical process undeniably hit close to the mark. Yet I would recommend to him that he read Blok's (1974) illuminating study of the Mafia in Sicily in order to disabuse himself of his fanciful belief that leaders and followers are all in it together and that patron-client relationships are essentially symmetrical ("to each according to his need" is his astonishing characterization of the allocation of resources between chieftains and their supporters). What Lewthwaite fails to realize is that the benign surface of clientage (in which, in his words, "inequality does not breed alienation") depends upon the fact that, as Wolf (1966) points out, both patron and client operate within the overarching structure of state institutions. When the state is present to guarantee the asymmetry of the patron-client tie, the patron can afford to maintain an appearance of unalloyed munificence. When the patron's ambitions and operations run counter to the state's prescriptions, however, he cannot avail himself of state power to certify his predominance and must himself enforce the asymmetry between himself and his supporters. This, as Blok explains, is the situation of the Mafia. Leaders face the same problem when state institutions are weak or nonexistent. Just as when his activities are opposed by the state, the leader must himself supply the violence necessary to discipline dissident followers. As Moore (1966:214) points out, "gangsterism is likely to crop up wherever the forces of law and order are weak. European feudalism was mainly gangsterism that had become society itself and acquired respectability through notions of chivalry." In the Bronze Age, of course, no state structures existed to maintain the power of the emergent elite over their followers. The elites would have had to supply their own enforcement, and the furniture of their burials suggests they gloried in that necessity. As Anderson (1974) has shown, furthermore, there is a direct historical link in Europe between feudalism and the barbarian social system which arose in the Bronze Age.

Many responses seem to find my theoretical position to be "polarized" (Adams) and "oversimple" (Sherratt). This is probably inevitable. As Moore (1966:522) has said,

> any simple straightforward truth about political institutions or events is bound to have polemical consequence. . . . In any society the dominant groups are the ones with the most to hide about the way society works. Very often, therefore, truthful analyses are bound to have a critical ring, to seem like exposures rather than objective statements. . . .

We are so mired in the theories and mythologies which justify the systems of stratification that surround us that a clear exposition of the origins of stratification in the remote past in terms of universal social processes cannot avoid seeming somehow too radical.

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